ENVIRONMENTAL ASSESSMENT BOARD

Ontario

ONTARIO HYDRO DEMAND/SUPPLY PLAN HEARINGS

VOLUME: 126

DATE: Wednesday, April 1, 1992

BEFORE:

HON. MR. JUSTICE E. SAUNDERS	Chairman
DR. G. CONNELL	Member
MS. G. PATTERSON	Member

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ENVIRONMENTAL ASSESSMENT BOARD
ONTARIO HYDRO DEMAND/SUPPLY PLAN HEARING

IN THE MATTER OF the Environmental Assessment Act, R.S.O. 1980, c. 140, as amended, and Regulations thereunder;

AND IN THE MATTER OF an undertaking by Ontario Hydro consisting of a program in respect of activities associated with meeting future electricity requirements in Ontario.

Held on the 5th Floor, 2200 Yonge Street, Toronto, Ontario, on Wednesday, the 1st day of April, 1992, commencing at 10:00 a.m.

VOLUME 126

B E F O R E :

THE HON. MR. JUSTICE E. SAUNDERS	Chairman
DR. G. CONNELL	Member
MS. G. PATTERSON	Member

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R. CUYLER		ON HIS OWN BEHALF
L. BULLOCK		CANADIAN NUCLEAR ASSOCIATION

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---Upon commencing at 10:03 a.m.

THE REGISTRAR: This hearing is now in session. Be seated, please.

THE CHAIRMAN: I would like to note on the record Exhibit 551, filed by the Independent Power Producers Society of Ontario, a document entitled Ontario/Manitoba Interconnection, Preferred Route, Draft.

---EXHIBIT NO. 551: Document entitled, Ontario/Manitoba Interconnection, Preferred Route, Draft, filed by IPPSO.

Mr. Hamer?

MR. HAMER: Thank you, Mr. Chairman.

DAVID WHILLANS,
KURT JOHANSEN,
FRANK CALVIN KING,
WILLIAM JOHN PENN,
IAN NICHOL DALY; Resumed

CROSS-EXAMINATION BY MR. HAMER (Cont'd):

Q. Just before we go back to the table on various sources of radiation, Mr. King, could we look for a moment again at the Hare Report excerpts which I have in my Volume 2, tab 14?

I am looking at a section which is headed Biographical Sketches of Commissioner Advisory Panel, et cetera, which is about four pages in from the front of the excerpts.

MR. KING: A. Sir, which page?

Q. It doesn't have a page number on it. If you count about four pages in it is headed Biographical Sketches.

A. Yes, I have it.

Q. And we can see, can we not, from that page and following that in addition to his own scientific background Dr. Hare had assistance from Dr. Ian Burton, who was a Professor of Geography at U of T?

A. He was a member of the Advisory Panel.

Q. And Dr. James Ham, who was a Professor of Science, Technology and Public Policy at the University of Toronto?

A. Yes.

Q. And Dr. John McGeachy, who is a Professor of Mechanical Engineering at Queen's?

A. Yes.

Q. And Dr. Vladimir Paskievici, who is Dean of Research and Graduate Studies at the Ecole Polytechnique de Montreal?

A. Yes.

Q. And he in particular is a recognized expert in the area of nuclear engineering?

A. Yes, he has been involved for many

years.

Q. And Dr. Alec T. Stewart, a Professor of Physics at Queen's University assisted Dr. Hare?

A. He was on the Advisory Group, yes.

Q. And Dr. Boris P. Stoicheff, who is a Professor of Physics at the University of Toronto?

A. Yes.

Q. And Mr. Ralph Torrie, who had been Assistant Director of the Energy Research Group of the United Nations University and the International Development and Research Centre?

A. Yes, that is what it says.

THE CHAIRMAN: Did you deliberately exclude Dr. Robert H. Haynes, New York University?

MR. HAMER: Not at all.

Q. And he was a member of the Society for Risk Analysis, correct, among other things?

MR. KING: A. Dr. Robert Haynes?

Q. Yes.

A. Yes. I should point out that is not the same institute we were talking about yesterday. That is --

Q. Correct. That is a North American body?

A. Yes.

Q. With membership both in the United States and Canada?

A. And in Europe. And international as well.

Q. Are you a member of that body?

A. I used to be.

Q. And the point I try to make is that we can all accept that in carrying out his investigations Dr. Hare had extensive technical and scientific support, did he not?

A. I believe so, yes.

Q. And, in addition, Dr. Hare had input from a number of public interest groups who functioned in effect as intervenors although it was not an adversarial hearing; is that correct?

A. He used the word intervenor to apply to anybody that was assisting, participating beyond the advisory group. It was a very broad use of the term.

Q. All right. If we turn over in my excerpts to a document headed Annex 1, again, unfortunately it is not page numbered. The second page is numbered page 198. You will find that it follows page 196 in the excerpts.

We find there, do we not, a table of the various intervenors, many of whom are also represented

in this hearing?

A. This is a table of consultants and intervenors.

Q. Yes. For example, my clients were represented there, the Canadian Environmental Law Association, the Canadian Nuclear Association, certain trade unions, the United Church of Canada, and Energy Probe?

A. Yes, I have found all of those on the table.

Q. And Environment Canada and Friends of the Earth?

A. Yes.

Q. And the International Institute of Concern for Public Health on the next page--

A. Yes.

Q. --submitted a brief?

A. Yes.

Q. Over on the next page, the New Democratic Party of Ontario through its MPP Brian Charlton?

A. Yes.

Q. The Nuclear Awareness Project, the Ontario Federation of Labour?

A. Yes.

Q. Science for Peace on the next page?

A. Yes.

Q. And the Solicitor General of Ontario?

A. The Ministry of, yes.

Q. Now, I don't have it in my excerpts - I don't think we need to turn it up - but, in addition, Dr. Hare conducted visits at various relevant locations across Canada and internationally in connection with this study of nuclear safety?

A. I am aware of some of the visits that he conducted, as well as members of his staff visits, where they traveled alone as well.

Q. And in Exhibit 184, which I don't think we need to pull out, there is an "Annex 6", Roman numeral 6, entitled "Site Visits Conducted by the Ontario Nuclear Safety Review", and among other places they went to my client's installations in Mississauga, Chalk River and Whiteshell in Manitoba, to New Brunswick Hydro; you would accept that?

A. New Brunswick Electric Power Commission should be the...

Q. And the Point Lepreau nuclear generating station in particular, according to this document?

A. Yes.

THE CHAIRMAN: I am not quite sure -- this is all very interesting, but I am not quite sure how it really is cross-examination. Just speaking for myself really, this is all in the report, and, of course, in the course of argument you can draw attention to this.

Of course, the whole treatment of Dr. Hare's report is going to be, I suppose, a matter of argument as to how we should deal with it. It is certainly there, and it has significance, but how we should deal with it in this particular proceeding is something I suppose the parties will want to address.

MR. HAMER: Mr. Chairman, I appreciate that and certainly it will be a matter of argument.

I may have been led astray by your comments that particular passages in documents that were not put to the witnesses and asked and their acknowledgement and agreement requested...

I would expect that it would be my client's position, and perhaps others' in argument at the end of this case, that Ontario Hydro as the proponent has relied on the Hare Commission Report quite justifiably, and that, as a matter of fact, that is a document and set of conclusions which although not binding on this Board can certainly be relied upon to

some extent by this Board.

I understand, and again I may be anticipating cross-examination of later intervenors, but I understand that some parties have criticized the Hare Commission by reason of its composition and the way in which it carried out its inquiries.

THE CHAIRMAN: Well, the Hare Report is at the moment evidence at this hearing because it is a document filed by the proponent and is here as evidence in contrast to some of the documents that are filed by the intervenors which may eventually become evidence but at the moment specific extracts are being used to elicit from the witnesses their views about whatever subject happens to be before them at the time.

There is a distinction there. For instance, a number of the exhibits that have been put forward by the intervenors for cross-examination I am sure will become part of their cases when the time comes and will have to be weighed like all the other material.

[10:15 a.m.]

MR. HAMER: Perhaps we could leave it this way, Mr. Chairman, to the extent that the Hare report contains statements of fact as opposed to statements of opinion, we can take it as read that

those facts are accepted by Ontario Hydro as the proponent.

THE CHAIRMAN: Well, I don't know whether Mr. Campbell has anything to say about that or not.

MR. B. CAMPBELL: For instance, dealing with the matter where and an appendix in the Hare Report states that the commission or its agents attended on certain sites and conducted certain conversations, we are certainly not going to dispute that. I am quite willing to accept that that is accurately reported by Dr. Hare.

MR. HAMER: And that the general process of his commission is accurately described in his report. There is, for example, quite a lengthy document by at commissioner manager, I believe her name is Margaret Grisdale describing how they went about their work and we can take simply take that as being a factual description of that inquiry.

THE CHAIRMAN: I detect at the end of the day what your client will be saying is that we should accept the findings of the Hare Report and why, because of all the matters you have suggesting to the witnesses, that it was a carefully worked out thing by highly qualified people who made extensively investigations. And I suppose there may be others who

will take a different position on that.

MR. HAMER: Yes.

THE CHAIRMAN: But in the narrow ambit of cross-examination, I am not sure, unless these witnesses have some special contribution to make, I am not sure that it is helpful at this stage to go through some of the matters that you have been dealing with.

MR. HAMER: Now that I look at it, I think I am finished anyway, Mr. Chairman, or almost.

THE CHAIRMAN: Well, at least we got this particular matter out on the record so people can deal with it as they see fit as we go along. That was really my objective.

MR. HAMER: Yes.

Q. Dr. Whillans, could we go back then, please, to the table which is contained in Exhibit 507 which I have excerpted at tab 8 of our Volume 2.

DR. WHILLANS: A. I have it.

Q. First of all, I believe you were going to check a couple of the entries on that table overnight dealing with the nuclear power occupational dosages and the nuclear fuel cycle.

A. Right. Well, with respect to the entry occupational nuclear power I would like to clarify that the number referred to here is the average

dose, effective dose for a member of the public, as the title of the whole table suggestions, as a result of nuclear power generation, and I think this is clear from the last paragraph on the previous page which is in Exhibit 507 but not --

Q. Which I haven't excerpted.

A. Not in your excerpt. Maybe I could read it.

It just refers to the radiation dose resulting from radionuclide emissions from nuclear station operation should be compared with the radiation dose received from the public from other sources and the variability of this background radiation.

The reason occupational was put in, I believe, was to distinguish it from other artificial sources such as medical and consumer products. But it does refer to dose to the general public as a result of nuclear power generation.

With respect to the fuel cycle --

Q. Can I just stop you there for a moment.

A. Yes.

Q. Not a great deal turns on it, but in deriving that annual average dose to members of the public, I take it that the authors included members of

the public who happened to be nuclear workers; is that correct?

A. Yes, nuclear workers are also members of the public. And this is an upper limit. As I said in the direct evidence, the maximum dose to a member of the public is about .05 millisieverts or less, and that's for a very restricted sub set of the population. The average dose is much less. And whether or not you include radiation workers doesn't make a lot of difference.

Q. Yes. And then nuclear fuel cycle?

A. Nuclear fuel cycle, I am not sure what your question was.

It is intended include all other parts the fuel cycle, including mining, milling, the frontend parts and also waste management.

Q. But not generation?

A. But not generation. But again, as it's an upper limit, it wouldn't make much difference if it were including generation.

Q. And do we take it then that for the whole of the nuclear fuel cycle including generation one sums those two figures as being the upper limit?

A. Yes, that would certainly give you a conservative upper limit I think.

THE CHAIRMAN: That would be .02.

DR. WHILLANS: .02, but it's still an upper limit.

My suspicion is that it would still be .01 but technically that's what the table says.

MR. HAMER: Q. And that's because of the symbols for less than on the left of each of those figures?

DR. WHILLANS: A. Yes.

The intent of this part of table was to give an upper limit which is defensible but also to contrast this with the other numbers which are much greater.

Q. And now turning back to the other sources of ionizing radiation on the list, I understand that the entry for internal sources relates to irradiation of the human body from elements which are actually present inside the body; is that correct?

A. That's right. And I think as I said in the direct evidence, the main contributor to this is the potassium 40, the isotope of potassium which is radioactive and which delivers doses to the body even though it's a natural part of the potassium that's in the body.

There are some other contributors which

are taken into the body as a result of diet or drinking water and so forth. The second largest contributor is part of the uranium decay chain, lead and polonium 210. The third large is perhaps carbon 14 but we are talking about .01 millisieverts or less.

So there is really one large contributor, another one which is somewhat smaller and a number of others which are very much smaller.

Q. In sum, those sources produce on average does by my arithmetic, 20 times the maximum average dose from the nuclear fuel cycle which we have just agreed on?

A. That's right.

Q. And then the entry for medical irradiation relates primarily to X-rays, I take it?

A. There is two categories showing X-rays or nuclear medicine, which is a set of diagnostic techniques that involve radioactive materials and...

Q. Barium enemas, that sort of thing?

A. Well, that might not be a best example.

Something like thallium scans for heart function, or technetium is one of the major, technetium 99M is one of the major imaging isotopes.

Q. And by my count those are again well over 20 times the maximum annual average dose resulting from the nuclear fuel cycle?

A. That's true.

Q. And consumer products, would that cover things like watches and items like that?

A. Yes. The major contributors are actually in tobacco, again the lead polonium 210, and there are a number of others things, tritium dials on watches, uranium isotopes in dinnerware. There is a large range of things.

Q. Just going back to radon for a moment. If one were to institute an energy conservation program whereby people's houses were sealed up more tightly than traditionally they were, one could anticipate enhancing the annual average doses to those members of the public who participated in such a program; is that correct?

A. Well, I think a fairer description would be to say that if you only seal up a house, and there a source of radon which can get into the house, that will, because the ventilation changes are smaller, that will certainly increase the dose. But there are other ways of reducing that exposure, by sealing up the source, for example.

Q. If one merely seals the house with those sticky tapes and that sort of thing, without taking steps to either remove the source or to improve the ventilation, that would be the effect, would it not.

A. If that's all you did, that would be the effect, yes.

Q. I would like to turn to occupational doses, and to begin with, may we look again at the excerpt from the Hare Report at tab 14, and I am looking at page 106 in that excerpt. It's paragraph No. 232 of the report to the Minister.

Do you have that?

A. Yes, I do.

Q. And we see that in paragraph 232, Dr. Hare or the authors have gone through a discussion of doses to workers in the previous paragraphs, and then in 232 he states:

> It is obvious, however, that these exposures, that is exposure to workers, are much larger than is typical of the public, even of those resident near the exclusion fences of the nuclear generating stations. If prolonged exposure to ionizing radiation carries

with it the penalty of greater proneness to disease, that fact should show up among Ontario Hydro's work force, the same should be true of AECL employees at Chalk River and in nearby town of Deep River where many AECL live.

[10:26 a.m.]

I take it you would agree with that logic?

A. Generally, yes.

Q. And another way of putting it is to say perhaps that the radiation workers are in some senses sentinels for the rest of the community; is that fair?

A. Yes, I think I could agree with that.

Q. And while we needn't turn it up, Exhibit 519 at page 35 tells us that doses to Ontario Hydro's workers have been declining substantially, and I believe you set out there the decline in average doses from 1981 onward and that has been quite substantial; correct?

A. Yes.

Q. In one of your interrogatory responses, which again we needn't turn up unless we need to to clarify, it is stated that the average Ontario Hydro worker receives one of the lowest

occupational doses amongst world utilities? Are you familiar with that?

A. Could you refer me to the interrogatory, please?

Q. Yes. If you turn to my tab 6 in Volume 2 I believe you will find it. This is tab 6 in my Volume 2, and it is Interrogatory 9.42.6.

THE REGISTRAR: It is given the number 520.40.

---EXHIBIT NO. 520.40: Interrogatory 9.42.6.

DR. WHILLANS: Yes, I have it.

MR. HAMER: Q. And if one looks at the bar graph headed Dose Per Gross Megawatt Year, which in effect compares worker doses on a basis which is made comparable on the basis of installed capacities in the various utilities, Ontario Hydro's dose is one of the lowest amongst those countries?

DR. WHILLANS: A. Yes, I agree with that. I want to make the distinction that we are looking at here is what I call collective dose.

Q. Right.

A. We had talked about individual dose previously.

Q. Some are higher? Right. So that some are higher and some are lower than is shown by

these bars, obviously?

A. Some...?

Q. Some individual workers?

A. Well, this is the sum of them all so, yes, they will vary around the average, yes.

Q. Right. Okay. Am I correct in thinking that Ontario Hydro in its nuclear stations exposes fewer workers per reactor than many of the other utilities who use nuclear generation? I think we see that in another graph.

A. Yes, I think two pages on in the same interrogatory response there is a set of bar charts of exposed workers per reactor.

Q. Yes.

A. And this is showing over a five-year trend, and Ontario Hydro is in the upper left-hand corner, and you can see -- well, barely, but the scales are the same for all the graphs, and Ontario Hydro's number of workers exposed even over the past five years is lower than most of the others, yes.

Q. In fairness, some of Ontario Hydro's exposed workers, however, are at relatively high levels of annual individual dose compared to some of the other average individual doses; is that correct?

I have put that inelegantly, but I think

we see that in one of other bar --

A. That's right.

Q. I think the one headed Annual Dose Per Exposed Worker?

A. Have you found that?

Q. It doesn't --

A. All right. On the next page. So what you say is true.

Q. But there are fewer workers at that relative high dose level?

A. That's right. And the first chart you referred me to, which is the collective dose comparison, is what multiplies the dose per worker times the number of workers and gives the overall impact on the work group in terms of dose.

Q. Right. And just while we have that interrogatory open, and if we go back to the bar chart headed Dose Per Gross Megawatt Year, Electrical, that is the one that shows that overall amongst its worker population Ontario Hydro is low in the world; correct?

A. Yes.

Q. And the comparison is also apt in the sense that the other countries shown on that bar chart are almost all light water reactor countries rather than heavy water reactor countries; is that right?

A. That's right. I think there is a note somewhere in this report to that effect.

Q. I think the answer to the interrogatory helps make the point?

A. Perhaps.

Q. The question was: What is the experience with regard to worker radiation doses resulting from CANDU stations compared to those resulting from light water reactors?

And in answer Hydro produced this series of graphs.

A. Right.

MR. KING: A. If I could just add for clarification that in the case of the United Kingdom it is primarily gas-cooled reactors, not water reactors.

Q. Right. And that country shows a dose per gross megawatt almost identical to Canada, or rather Ontario Hydro?

A. Yes. My understanding is that gas-cooled reactors have an advantage with respect to dose because you don't get some of the sort of corrosion that would occur and the pickup of radioactive materials that would occur in a water reactor.

Q. We can agree that it is almost

identical to Ontario Hydro's average? 21 is equal or almost equal to 20?

A. Yes, I was just adding some clarification on why gas-cooled reactors have low numbers.

Q. Mr. King, you would agree that .20 is almost the same as .21?

A. Yes.

Q. Thank you. Now, Dr. Whillans, the worker population which produces these statistics, as I understand it, actually has their doses measured with those little contraptions that we wore at Darlington? What do you call them?

DR. WHILLANS: A. Well, the TLDs? No?

Q. What's that?

A. They wear a badge which measures external dose.

Q. Yes?

A. And because internal dose from tritium is a major part of the doses in Ontario Hydro facilities they also submit regular bioassay urine samples, and these are analyzed just to make the internal dose.

Q. And they are kept on a dose register, then?

A. That's right.

Q. Now, if we go to tab 4 of my Volume 2 -- this may be an exhibit. I'm sorry, I haven't checked it. It is interrogatory 9.22.32.

THE REGISTRAR: .32?

MR. HAMER: Yes.

THE REGISTRAR: That is .41.

THE CHAIRMAN: 41?

THE REGISTRAR: Yes.

THE CHAIRMAN: Thank you.

MR. HAMER: Q. That interrogatory attaches a report summarizing the mortality experience of Ontario Hydro workers; correct?

DR. WHILLANS: A. That's correct.

Q. And somewhere in the report I think the statement is set out that Ontario Hydro was one of the first corporations in the world to set up a comprehensive system to record the mortality of its workers; is that correct?

A. I'm sorry, I am not familiar with that statement.

Q. If you turn to page 40 at the back - do you see it - in the first full paragraph, the last four lines:

Ontario Hydro thus became one of the

first companies in the world to establish any form of routine mortality monitoring program of its employees.

A. Yes, I see that.

Q. And you are aware of that?

A. I am now.

Q. And there are still only a handful of such companies. I take it you wouldn't be able to assist us on whether that is true or not.

A. Yes, I think only to comment that studies, as you know, are done at AECL and in large companies, but I think that would probably be just a handful, yes.

Q. We will come back to the AECL studies.

If you go to page 1 of the report -- and before we highlight some of the material items covered in the report, is there another witness on the Panel that can speak better to this report or is this something that is within your area, Dr. Whillans?

A. I think it is in my area.

Q. What has happened as of the time of this report is that 19 years of mortality experience of the male employees of Ontario Hydro who are either already pensioned or pensionable has been surveyed;

correct?

A. Yes.

Q. And on page 3 there is an explanation of standardized mortality ratios, and that is a typical statistic which is used in monitoring health effects of various kinds of industrial activity; is that correct?

A. Yes.

Q. And basically, all it does is take the actual deaths occurring in a particular category and comparing that to the number of deaths which are expected to occur in a like category based on historical data; is that correct?

A. Yes. I think here it says in this particular case: based on provincial death rates for the same age distribution during approximately the same period of time.

Q. So that, for example, one compares I think it is men between the age of 50 and 55 rather than simply comparing the entire male population?

A. That is right. It is age standardized.

Q. Right.

THE REGISTRAR: Mr. Chairman, I have just done another check, and I find that 9.22.32 was previously entered, and it is 520.12.

THE CHAIRMAN: Thank you.

THE REGISTRAR: So 41 is still vacant.

THE CHAIRMAN: Thank you.

MR. HAMER: Q. If we go to page 6 of this report in the second last full paragraph we find the statement that:

> There have now been 3,479 deaths recorded and all standard mortality ratios are below 100, reflecting the usual so-called healthy worker effect.

That means that the actual numbers of deaths observed in each category has been less than the expected numbers of death?

DR. WHILLANS: A. Based on the provincial average, yes.

Q. And the healthy worker effect is basically a phenomenon which is perceived whereby groups of employees in certain enterprises may in effect be self-selecting so that that population is healthier in general than the population at large; is that a fair description?

[10:40 a.m.]

A. I think that's fair. It is a very common phenomenon in occupation studies because the selection that occurs when someone has to come to work

every day is a very strong one in terms of health.

Q. You tend to be healthier if you go to work?

A. You tend to be healthier if you can keep a job, yes.

Q. And then we turn on page 8 to the subject of cancer in men on the dose register. Am I correct in understanding that, first of all, apart from lung cancer there weren't enough deaths from other kinds of cancer to come up with terribly meaningful statistics?

A. I think I referred to that in my direct evidence.

Q. And then the author says in the last five lines on page 8:

> The only possible exception is lung cancer which with 24 cases is the only category where the numbers are large enough that a dose response effect might be visible.

Then over on page 9 the author deals in the paragraph numbered 1 with the fact:

> That there is no evidence of a dose response gradient for long cancer.

Correct?

A. Yes, that's what it says.

Q. If you look to the table at the top of the page, you see that for workers who had a zero millisieverts dose, the standard mortality ratio is 139, which is more than expected, in quotation marks?

A. Yes.

Q. And then if you increase the dose to the gradient up to 50 millisieverts, it drops to 124, standard mortality ratio.

Do you see that?

A. Yes. My copy is not very clear, actually.

Q. I'm sorry.

The dose is set out in the less than 50 at the top?

A. I see that.

Q. And at the bottom it's dropped from 139 to 134. And then if you go to the gradient between 50 to 99 it drops again to 82, which is less than expected; correct?

A. Yes.

Q. And then between 100 and 149 millisieverts it goes back up to 101?

A. Yes.

Q. And between 150 and 199 millisieverts

it goes up again to 278, standard mortality ratio?

A. Yes.

Q. And then the highest dose level, it falls right off to zero?

A. Yes.

Q. And that's what the authors means when they talk about the absence of a dose response gradient?

A. Yes.

Just as comment, I was looking as you were talking I don't believe that these statistics are corrected for smoking versus non-smoking and that's by far the most important determinant of lung cancer. And if that is true, I would think that these numbers do not reflect anything to do with doses.

Q. Let us go over then to page 11 and deal with leukaemia deaths. The last full paragraph on that page begins:

> It is of interest that leukaemia deaths remain well below the number expected in the general population of the same age distribution.

And then a little further on in the paragraph:

> The lack of any excess leukaemia in

this group of radiation workers is not in itself surprising since from what is already known of the effect of ionizing radiation, the average dose and thus total -- and it should say person sieverts -- has not been large enough to cause any detectable increase in the leukaemia deaths in a group of this size. Et cetera.

You would agree with that logic and that conclusion?

A. I would.

Q. And then the statement is set out on page 12 in the first full paragraph:

> In view of the mounting public anxiety about a possible causal relationship between electromagnetic fields and leukaemia, it is of interest that Hydro radiation workers, many of whom will have been exposed in nuclear generating stations to both ionizing radiation and electromagnetic fields have shown no evidence of excess leukaemia.

And then finally:

> Similarly, there has so far been no

death from multiple myeloma, a disease that has shown a positive relation to ionizing radiation in some large scale studies.

And that is it a significant finding, is it not, in terms of the effects of radiation Ontario Hydro's workers?

A. Yes. I think as I mentioned in the direct evidence, when we look at the tables of numbers, there are so few - and I think we have already reached this point this morning - so few deaths in individual categories that it would be difficult to say anything. But certainly, I agree with their conclusions here.

I would also point out that this is a report up to 1988 and there are some additional studies being carried out now particularly with respect to electromagnetic field exposure and the results of those are not available. But there are some ongoing studies that extend this work.

Q. And this is something that it is prudent to study all the time on ongoing basis?

A. We believe so.

Q. And if cause for concern emerges out of the ongoing study, steps will be taken to deal with that concern?

A. Yes.

Q. In fact one of your jobs at Hydro is to make recommendations as to whether or not the evolving science indicates the need for a change in radiation protection practices; correct?

A. Yes.

Q. If we go over to page 20 of this report, we find a table which as I understand it compares workers and their mortality in accordance with the area within the corporation in which they were most recently employed.

Do you have that?

A. Yes.

Q. And that breaks the mortality down amongst the nuclear division, the thermal division, and so-called other areas in the corporation; correct?

A. That's correct.

Q. We can see that the first category of causes of death broken out there is cancers, and that would be the one of most significant from the point of view of radiation effects; correct?

A. Certainly that is the one that we would look at first, based on the evidence today.

Q. We find if we look to the total column on the right, that the incidence of cancers is

less than expected with the standard mortality ratio of 84?

A. Yes.

Q. And the highest incidence of cancers is in the thermal division with the standard mortality ratio of 93?

A. Yes.

Q. And the next highest is in the "other" areas of the corporation with the standard mortality ratio of 84?

A. Yes.

Q. And the lowest is the nuclear division at 62 standard mortality ratio?

A. Yes. The row just underneath those numbers is marked CL, that's the confidence limits on that. For example, the 62 for nuclear has confidence limits going from 40 to 91. The point I was going to make is that if you look at the confidence limits on all of those individual numbers, they are not really different.

Q. Well, except that the confidence band for the thermal division has an end point in excess of 100.

A. That's true.

Q. And that's significant.

A. That is significant, yes.

Q. It's one thing that the epidemiologist watches for?

A. Yes.

What this means is that, for example, the nuclear value of 62 is statistically significantly less than one. That's not true for thermal.

Q. Right. And the confidence band means that for the nuclear workers, for example, one can have 95 per cent confidence that the true mortality ratio, if you like, is somewhere between 40 and 92; is that fair?

A. Yes, in the statistical sense that's exactly what it means.

Q. And that's all we are talking about is statistics?

A. Right.

Q. Right. And we will talk a little later about damn lies?

A. Okay.

Q. And if we look to the line at the bottom of that table, we see that for all causes of death the standard mortality ratio for the nuclear workers is again lower than for those in the other areas of the corporation, both thermal and "other";

correct?

A. Correct.

Q. And all of the Ontario Hydro workers have standard mortality ratios in respect of which the confidence band is below one when one looks at all causes of death?

A. That's right, that's healthy worker effect.

Q. And the nuclear workers have the lowest situated confidence band being 49 at the bottom end and 73 at the top end; correct?

A. Yes, that's correct.

Q. Putting that another way, we can say that in summary, Ontario Hydro's workers are outliving the rest of the population? And I am speaking of quantity of life, not quality of life.

A. It's a big step. [Laughter]

I am hesitating because for people who are still alive, I am not sure that you can say they are necessarily outliving the rest of the population. But certainly within this time frame, mortality experience is lower than the rest of the population, I would agree with that.

Q. And the nuclear workers' mortality experience is lower than in other areas of the

corporation?

A. Yes.

Q. If we go back to an excerpt from the Hare Report at tab 14, I would like to go to the lower case Roman numeral page numbers and look at Roman numeral 15, (xv).

A. I have it.

Q. And I take it that you agreed with Dr. Hare's conclusion at the bottom of that page under Health Matters and you agree today that:

> There is no evidence that the normal operation of Ontario Hydro's reactors has caused or will in future cause harmful effects in either the reactor work-force, which is by far the most supposed group, or the general public.

You agreed with that conclusion when it came out and you agree with it today?

A. Well, we addressed this point I think in an earlier cross-examination. I agree that there is no evidence that we have caused harmful effects in either the work-force or the general public, that's true.

Q. And you would also agree with his concluding remark, vigilance is required?

A. Yes.

Q. And we will come to some of the work which is ongoing with respect to some concerns that have arisen.

For example, at Roman numeral 16, in the second paragraph, the last sentence indicates:

> It is, however, too early for all latent cancers to have been revealed. So of course the corporation will continue to monitor cancer mortality amongst its work-force?

A. Yes.

Q. And in the next paragraph Dr. Hare says:

> Epidemiological analysis of the exposed workers of AECL is carried out. It is a longer and larger sample. It too shows cancer mortality to be below that in the general public, although for Chalk River employees it has tended to rise in the past 15 years and is now level with or marginally above that of the public.

And that was written in 1987 or 1988?

A. That's right.

Q. And then Dr. Hare says in the next

paragraph:

> There is no comparable study of public impact in Canada. Public exposure to radiation is at least several hundred times smaller than in either AECL or Ontario Hydro work-forces, hence measurable effects are unlikely.

And again that is the sentinel logic at work; correct?

A. Yes.

Q. And we will see in a few moments, if we turn to tab 3 in my Volume 2, at tab 3 we find an interrogatory answer submitted by Hydro, No. 9.22.30.

THE REGISTRAR: That has been previous entered as 520.13.

MR. HAMER: Q. And that is a study which was published by some epidemiologists at my client's Chalk River facilities dealing with mortality among long-term Chalk River employees as of 1986; correct.

DR. WHILLANS: A. Yes. I am not sure they are both epidemiologists, but the rest of what you say is correct.

Q. I think they are health physicists as a matter of fact?

A. Health statisticians perhaps.

[10:55 a.m.]

Q. And we can see from the abstract that no statistically significant increases in cancer deaths were found in any of the groups analyzed?

A. That's correct.

Q. If we go over to page 3 we find table 3, again dealing with standard mortality ratios for major causes of death amongst males who had been employed by AECL at Chalk River where there are nuclear reactors; correct?

A. That's correct.

Q. And if we look at the line in Table 3 headed Cancer, we see five-year intervals for standard mortality ratios: 1966 to 1970, and then 1971 to 1975, and so forth?

A. Table 3? Sorry, yes.

Q. And in that line headed Cancer, we see that for the first five-year interval the standard mortality ratio was .95, which is 95 in other people's parlance?

A. Yes.

Q. And then it dropped in the next five-year interval to .72?

A. Yes.

Q. But then it rose in the next interval

to .89 and again in the next interval to 1.07?

A. Yes.

Q. And that is the increase which Dr. Hare had remarked upon in the excerpt from his report which we were just referring to?

A. Yes. What we don't have here are the confidence limits, the 95 per cent confidence limits as were shown on the table 2 above, but if they are similar you could see that the differences are not significant, in some cases anyway.

Q. Well, in fact, what we are looking at here is probably an aging population to some extent; is that correct? These are not age-adjusted figures; these are mortality figures overall for that population. Correct?

A. Well, without reading the methodology I can't be sure of that, but there has to be some correction done when you calculate an expected number.

Q. All right. And you would take into account the ages at which the --

A. I would have expected so. I guess we could check that.

Q. Well, I don't think it is important.

What I want to get to is the more recent report which I have included at tab 3 but which was not

part of your interrogatory answer, and I think we find in Dr. Hare's Report that he had recommended continuing monitoring of the AECL employees in view of that increase that we have just observed in cancer mortality; correct?

A. Yes.

Q. Do we not find a further report published by the same authors which I have inserted at tab 3 behind your interrogatory answer?

A. Yes. I see it.

Q. And that is headed Cause of Death Among Long-Term Employees of Chalk River Laboratories, 1966 to 1989?

A. Yes. This is AECL Report 10293.

Q. And you recognize that document as one that would come to your attention?

A. Until you provided it I haven't seen this document, no.

Q. Do you have any reason to believe that it is not published by those authors under those auspices?

A. No, I know that they do keep an ongoing review, and this would be a natural update.

THE CHAIRMAN: This document should be an exhibit, I take it?

MR. HAMER: I would ask that it be marked, Mr. Chairman.

THE REGISTRAR: What page are we on?

THE CHAIRMAN: Well, it is halfway through tab 3. It is at the back of the interrogatory.

MR. HAMER: Mr. Chairman, I would be happy to supply a loose copy if that makes it more convenient for the Registrar.

THE REGISTRAR: Just as long as I know where I am going. So the next exhibit number is 552.

---EXHIBIT NO. 552: Document headed Cause of Death Among Long-Term Employees of Chalk River Laboratories, 1966 to 1989, AECL Report 10293.

MR. HAMER: Q. And we see at page 3 that the authors indicate in that short paragraph in the middle of page 3 in the last sentence in that short paragraph:

> The trend upward that was noted in the Hare Report for cancer deaths for the years 1971 to 1985 has not continued in the next four years to 1989. This suggests that the increases were normal statistical variations.

DR. WHILLANS: A. I see that, yes.

Q. And you would accept that as a logical conclusion, assuming that those increases did

not continue?

A. Yes, because that is what I was trying to point out when we were looking at the previous table. There really were not statistically significant differences amongst the years that were referred to by Dr. Hare.

Q. Right. Well, turn to page 10, please, and you will find the confidence intervals have been thoughtfully supplied for you there, and this table sets the numbers out vertically rather than horizontally, but on the right-hand side we see the standard mortality ratios with 95 per cent confidence intervals in brackets, and we see that for cancer deaths the standard mortality ratio is .89 with an interval of .76 to 1.05?

A. Yes.

Q. And then if we go to page 12 we see the horizontal table again that we saw in the previous report, table 3?

A. Yes.

Q. And we see the numbers again for those five-year intervals except that another five-year interval has been added, being the years 1986 to 1990?

A. Yes.

Q. And this time the standard mortality

ratio has dropped from 1.09 to 0.72?

A. Yes.

Q. And that would indicate that there is reason to be reassured that there is no upward trend emerging in cancer deaths amongst those AECL workers?

A. I think that is so.

Q. Yes.

DR. CONNELL: There have apparently been some corrections or changes in figures for the earlier periods.

MR. HAMER: I think we may find that discussed in the body of the report. I can't put my finger on that part of the discussion immediately, Dr. Connell.

DR. CONNELL: The biggest change seems to be in the cardiovascular figure for 1976 to 1980.

DR. WHILLANS: There also seem to be some changes in the cancer numbers for the period '76 to '80 and '81 to '85, but they are small changes.

MR. HAMER: Q. And as the health statistician refines the statistical methods and the data collection methods one anticipates seeing small changes like that in follow-up reports?

DR. WHILLANS: A. I think that is true.

Q. And if we look at, while we have it

open, tables 4 and following in that report on page 12 we see table 4 covers mortality among 562 AECL participants in the NRX cleanup, and that refers to an accident which occurred in 1952 or '53 at Chalk River; correct?

A. I think the dates and the circumstances of the accidents are described somewhere in the text.

Q. You are probably quite right.

A. On page 5 of this present report there is some comment about the NRX and NRU accidents.

Q. Would you accept that the NRX accident was in about 1952 or 1953?

A. Approximately that time, yes.

Q. Yes. And we see that deaths from all causes for that group of 562 participants has a standard mortality ratio of 0.83 with a confidence limit of .73 to .94?

A. Referring to...?

Q. Table 4, Deaths from All Causes?

A. Oh, from all causes.

Q. Yes.

A. Yes. Yes.

Q. If you look at the individual causes you see that they all have a standard mortality ratio

below 1 with the upper limits of the confidence bands being not significantly above 1; is that fair?

A. Could you repeat that, please? The upper limits of the confidence band not being significantly above 1?

Q. Let's deal with the first barrel first.

A. Yes?

Q. The standard mortality ratios are all below 1?

A. Yes.

Q. And the confidence intervals for the standard mortality ratios from the various individual causes of death have upper limits which in no case are significantly above 1; is that fair?

A. I don't think this table tells you that, no. I think the numbers say that the confidence range around each of those mortality ratios with the exception of other causes does include 1.

Q. Yes.

A. But it also includes lower values.

Q. Right. But in terms of health statistics, for example, the cancer confidence interval of .67 to 1.16 would not give you cause for undue alarm, would it?

A. Well, to me it means that the number is approximately equal to whatever basis was used here, presumably the provincial average.

Q. One doesn't see a sudden --

A. It is not significantly greater, that's right.

Q. Right.

A. I guess what you are getting at is if the number were higher than 1 and the lower confidence limit were higher than 1, then the value is statistically greater than 1. And that is not the case here.

Q. All right. And one sees similar figures in terms of the concepts that you and I have been discussing in table 5 for mortality among 533 AECL participants in the NRU cleanup which followed an accident in about 1957 or '58?

A. Yes, I think it says May, '58 on page 5.

Q. Finally, table 6 deals with mortality up until 1989 among 400 or so AECL employees who had lifetime doses in excess of .2 sieverts by 1982; correct?

A. Yes.

Q. And one sees nothing alarming about

the standard mortality ratios for deaths from any of the causes listed there?

A. Yes.

Q. The recorded accumulated lifetime doses in excess of .2 sieverts would mean that that group is a high exposure group, if I can put it that way; is that fair?

A. That would be the highest group by dose interval, yes.

Q. I want to deal briefly with dose limits, I believe is the term, established by the ICRP.

A. Yes.

Q. You are familiar with those?

A. Yes.

Q. What does that stand for?

A. The ICRP?

Q. Yes.

A. It is the International Commission on Radiological Protection.

Q. And I think you told us something about that in your testimony in chief?

A. Yes, I did.

Q. And would you turn to the Hare Commission excerpts again, please, at tab 14?

I take it that in your position you would

be familiar with the criticisms which have been circulated concerning dose limits established by the ICRP?

A. I have certainly heard some criticisms. I don't know if I have heard them all.

Q. And it is not surprising to you that from time to time over the years those dose limits are revised--

A. No, that is not surprising.

Q. --as our knowledge improves?

A. Yes.

Q. And are you familiar with the criticisms of the ICRP dose limits which have been advanced by Dr. Rosalie Bertell?

A. I have heard some criticisms from Dr. Bertell.

Q. And we see that Dr. Hare had a submission filed by Dr. Bertell on behalf of an organization whose initials are IICPH, and I think we saw that in the list of intervenors that I ran through with Mr. King?

A. I don't remember whether we saw it actually.

Q. All right. We will take it as read. And we see in paragraph 9 of the excerpt -- I'm sorry,

have I directed you to the page?

A. No.

Q. Page 218, please.

A. I have it.

Q. We see that in paragraph 9 Dr. Hare deals with the IICPH, which is the International Institute of Concern for Public Health, whose principal spokesperson is Dr. Bertell.

A. Yes.

Q. You would agree that Dr. Bertell is well-known for her crusading work to improve, as she sees it, standards of radiological protection worldwide?

A. Yes.

Q. In the next paragraph we see that her submission was circulated to Ontario Hydro and AECL for comment?

A. That is what it says.

Q. And both corporations responded in depth and detail, and those responses were sent to Dr. Bertell who in turn had replied?

A. That is what it says.

Q. I wanted to ask you if you were part of formulating the response to Dr. Bertell's submissions on behalf of Ontario Hydro.

A. This would have been some five years ago. I can't honestly say that I remember contributing to them. It may have been in a fairly informal way. You know, the comments may have been coordinated by someone and I gave comments to him, but I can't honestly remember that.

Q. Let us attempt to refresh your memory with some of the further comments from Dr. Hare's report. He says in paragraph 11:

> Dr. Bertell's main thrust is that the available evidence on dose/response relationships resulting from radiation exposure is being misinterpreted by the scientists who dominate the regulating and standard setting bodies, most notably ICRP.

I take it you were aware of criticisms like that being advanced?

A. Yes, I have heard that.

Q. And she says she is especially critical of the role played by physicists in establishing protective standards and argues that medical and health professionals are being excluded from a proper role in the regulating bodies. She is also critical of AECB because it lacks staff with such

qualifications. She herself is a mathematician and biometrician.

A. That is what it says.

Q. You would accept that that is Dr. Bertell's background and the criticisms that she was advancing at that time?

A. She does have a Ph.D. in Mathematics, and I have heard comments that she has made which are similar to this, yes.

Q. And in paragraph 12 Dr. Hare states:

> I cannot agree with the submission's recommendation 'that Canada no longer rely on ICRP, UNSCEAR, or BEIR as the scientific support for radiation protection standards.'

And you would agree with his rejection of that proposal, would you not?

A. Yes, in my direct evidence I relied on those three groups in my studies.

Q. And Dr. Hare goes on, and you would agree with his conclusion that:

> On the contrary, it is essential that Canada be guided by the findings of these bodies.

A. Yes.

Q. And they are accepted by the world scientific community as the best clearing houses for the empirical data that are available as the authoritative bodies to judge the meaning of the evidence, and, in the case of ICRP, to suggest standards for safe exposure.

You would agree with that?

A. I do.

Q. And you would further agree that:

Canada, Ontario and Ontario Hydro need not be bound in a legal sense by what these bodies find, but that they would be ill-advised to abandon them as the best sources of advice and intellectual authority?

[11:15 a.m.]

A. Yes. It might be useful to add that the ICRP itself believes that it is providing guidance which can then adopted by national authorities and their legislation. They don't expect that everybody is going to take their recommendations exactly as stated because circumstances differ in different countries and there needs to be more emphasis in one area than another and so forth. But as it says here, they are

the best sources of advice and the word here is intellectual authority.

Q. In fact, as a general rule, Ontario Hydro tries to beat their guidelines by a substantial margin, do they not?

A. Well, Ontario Hydro has guidelines for radiation protection which are more protective than what is recommended in the current ICRP, for example. So in that sense I guess you're right.

Q. You would agree with me.

A. I wouldn't say we try to beat them. It's not a competition to have the lowest.

Q. Pardon my inelegance.

Dr. Hare goes on to describe the IICPH's doubts about ICRP which takes the form of allegations that is ICRP is biased by the origins of its members.

IICPH asserts that ICRP is dominated by physicists and medial administrators, many of them "involved in national atomic energy development", and that ICRP cannot therefore be considered free of all "bias conflict or government pressure".

The quotations being taken from their submission.

Persons qualified in occupational and

public health have been excluded from membership since its conception in 1950.

And you would recognize that as a criticism of the kind advanced by Dr. Bertell?

A. Yes, I have heard that criticism.

Q. And Dr. Hare goes on to reject that criticism as well in paragraph 15 in saying:

> In fact, ICRP's work is done principally by four expert committees whose composition does include eminently qualified individuals in the appropriate disciplines.

The committee chairpersons are also members of ICRP itself, you are aware of that?

A. Yes.

Q. And then two of the individuals specifically mentioned by IICPH has having been "deliberately excluded" by ICRP have in fact served on these committees and are amongst the authors of ICRP documents.

A. I don't know who those two individuals are.

Q. Second, scientists of sufficient eminence to be appointed international serve in their person capacities.

Regardless of their affiliations they are expected to use their scientific skills objectively and the whole ethos science dictates that they try to do so.

I believe that they succeed and you would believe that too, would you not?

A. Yes.

Q. And then over at page 221, I'm sorry I should start at the bottom of page 220, Dr. Hare refers to the fact that:

> ICRP's style is indeed laconic, and it's pronouncements are often made ex cathedra in a way that I myself austere. ICRP without any doubt represents the consensus of those most qualified to make such judgments according to the accepted standards of science, the same is true of UNSCEAR and --

Do you call that BEIR 5?

A. BEIR 5, yes.

Q. And that's similar to the opinions that you accepted a moment ago?

A. That's true.

Q. With respect to those bodies?

A. Yes.

Q. He goes on to say:

I believe that this comment is also valid for Canada's national situation in radiological protection. Our own institutions are responsible and highly competent yet they are in danger of losing public support because of unsubstantiated but widely disseminated criticisms.

That is a strong statement.

A. Yes, it is.

Q. But you would agree with it, would you not?

A. Generally, yes.

Q. Yes. And then if we go back to the Roman numeral excerpts which set out Dr. Hare's recommendations, to Roman numeral 18.

A. Yes.

Q. It's called Commission Recommendation 11.2, I want to ask you if you agree with this recommendation:

In spite of many ill-informed allegations, the International Commission on Radiological Protection remains the best available body for the determination

of radiological dose limits. AECB should continue to base its regulations on ICRP guidelines, although not necessarily according to it's timetable. Provincial practice should follow suit.

You would agree with that?

A. Yes. I think I might reword the first, it's the best available source of information about radiological dose limits.

Q. Fair enough.

A. As I said previously, I think we do expect that they will be customized to the particular Canadian situation.

Q. So they don't determine your dose limits, they provide guidance and you determine the does limits under the AECB's --

A. They provide guidance and the AECB determines dose limits, yes

Q. And you follow those?

A. Yes.

Q. If we could go to page 132, arabic numbers, of the Hare excerpts.

A. Yes.

Dr. Hare deals with the ALARA concept and I believe you dealt with that in chief as well, did you

not?

A. I did mention it, yes.

Q. And just to remind everyone, the ALARA principle states that exposure to radioactivity should be as low as reasonably achievable, social and economic factors being taken into account; correct?

A. Yes.

Q. And Dr. Hare recalls in paragraph 266 that in his own childhood he recalled road safety objectives in his native country being specified in similar terms.

And over at page 133, paragraph 270, having referred to the Layfield inquiry in England, Dr. Hare concludes in paragraph 270:

> In Ontario Hydro's case I am satisfied that ALARA has been both a useful discipline and active principle in decision-making, but the weighing of cost and benefit never ceases.

You would agree with that?

A. Yes.

Q. And he goes on to give an example:

> The problem arose in the question of a second shutdown system at Pickering A. Was it worth investing a large sum of

money and incurring substantial worker radiation exposure to increase safety marginally? The probability that second shutdown system would ever be needed to prevent a serious accident was judged to be extremely low. A much greater gain in safety could be achieved by investing the same amount of money and worker exposure in areas where the threat to safety is higher.

And that's the kind of balancing that you and your colleagues in other disciplines go through all the time in Ontario Hydro?

A. Yes. Maybe Mr. King would like to comment about that particular one.

Q. Well, Mr. King, if I could ask you about the next paragraph, 271:

This kind of cost/benefit and risk/benefit analysis can be quantified and applied as a formal discipline.

You are aware of that?

MR. KING: A. Yes, I am.

Q. And he goes on to say:

Doing so involves assigning values to human life, health and injury in a

fashion unwelcome to many. It also leads directly to a mechanism for making comparisons with other modes of energy production, or its avoidance, for energy conservation is, in many ways, a form of production.

Such choices confront the province now and you would agree, Mr. King, or perhaps even Mr. Penn, that those kinds of choices confront us in this hearing as well.

A. The only point I would like to make with respect to that paragraph is the assigning values to human life. You can get substantial benefit in a cost/benefit risk/benefit analysis and you can avoid that and not assign a dollar value. You can express pros and cons in other ways, because as you probably know, it's quite a controversial subject.

Q. But my question was: You would agree that those kinds of choices still confront us in this hearing, and I am referring to the choices which Dr. Hare referred to in paragraph 271?

A. Yes, I think these choices are always present.

Q. And for example, one can not only use this kind of cost/benefit or risk/benefit analysis to

compare modes of information of energy production, one can apply the same analysis to energy conservation?

A. I would think whenever you have choices before you, you have to look at all the choices with a methodology which brings out all the pros and cons.

Q. So the answer is yes?

A. Yes.

Q. Thank you.

Dr. Hare goes on at paragraph 272 to state:

> If ALARA is a useful discipline when wise people use it, it may also become a counter-productive weapon. If applied without discretion action it may lead to ratcheting - an inevitable increase in safety standards whether or not this is justified. As a result, wrote Layfield, national resources may be misallocated towards nuclear safety and the economics of nuclear power may be unreasonably handicapped.

Perhaps, Mr. Penn, I could ask you if you would agree with that logic and that observation?

MR. PENN: A. Well, I think all I can

give is a personal view on this matter.

Q. That would be fine.

A. The ALARA principle implies that if a situation is practical from a physical point of view, and a change may lead to some small improvement, that then it should be encouraged to be done.

Clearly there is a need to balance the expenditure that would be made to make perhaps a small improvement with that same expenditure that could be made in other areas.

And that is a matter of judgment and analysis.

Q. And you would agree, however, that there is a danger of ratcheting too high or too low, depending on how you look at it in terms of the benefits achieved through the expenditure?

A. Well, I think we are generalizing now. We really have to look at every case on its own merits. But it's my observation that in some jurisdictions there has been a tendency towards ratcheting.

Q. I am going to turn to our Volume 3 of materials for Dr. Whillans. I don't know if this would be a convenient time, Mr. Chairman.

THE CHAIRMAN: We will break for 15

minutes.

MR. HAMER: Thank you.

THE REGISTRAR: Please come to order. This hearing will recess for 15 minutes.

---Recess at 11:30 a.m.

---On resuming at 11:50 a.m.

THE REGISTRAR: Please come to order. This hearing is again in session. Please be seated.

THE CHAIRMAN: Mr. Hamer?

MR. HAMER: Thank you Mr. Chairman.

Q. I am going to refer, Dr. Whillans, to our Volume 3, tab 6, which is the slimmer volume.

DR. WHILLANS: A. Yes.

Q. This is a publication of the American Medical Association, and as I understand it, it summarizes a larger report which had been prepared by the Council on Scientific Affairs of the American Medical Association.

Have you had an opportunity to review this article which I have provided to you a day or so ago?

A. Yes, I looked through it last night.

Q. Thank you.

THE CHAIRMAN: Should this be given an Exhibit No.?

MR. HAMER: Yes, please.

THE REGISTRAR: The No. 553.

THE CHAIRMAN: Thank you.

---EXHIBIT NO. 553: Document entitled "Medical Perspective on Nuclear Power", Council on Scientific Affairs.

MR. HAMER: Q. I appreciate, Dr. Whillans, that you are not a medical doctor, but as someone experienced in the area of health effects of certain processes, I would like to ask you some questions about some of the recommendations that are made by the American Medical Association to its members.

DR. WHILLANS: A. Yes.

I think I should point out that this is a report prepared by an expert committee and they list on the first page who the members of the Committee were, and they are certainly people well experienced in radiation protection. They are not all medical people though.

Q. All right. The point I want to draw in general from this document is that as I understand it, one of the difficulties faced by the nuclear industry, if I can call it that, is in having the scientific community communicate its understanding of nuclear power to lay persons and members of the public;

is that fair?

A. I think that's fair.

Q. And the American Medical Association experts say on page 2728 of this report a number of things about nuclear power in relation to other forms of power generation, and in the middle column under the heading Risks Related to Nuclear Power we see this observation, I would ask for your opinion on that:

> Generating electricity by my mean entails some risk; for instance, 166 persons died in a July 1988 explosion on a North Sea oil rig, underground coal mining is one of the most hazardous occupations, and in of the United States approximately 100 persons are killed annually at grade crossings during the transport of coal to power plants. Emissions from the combustion of coal contribute to air pollution and disease and the ash and residue of coal combustion must be disposed of. All of these activities involve risk.

And you would agree with that as a general proposition?

A. As a general proposition, yes. I

don't know some of these specific numbers.

Q. But in terms of someone who assesses the health effects of nuclear power generation and its related activities, you do have some professional awareness of the fact that comparisons are drawn between those risks and the risks attended upon other forms of industrial activity?

A. Yes, I am aware that happens, yes.

Q. And, for example, lower down in the same column we see a paragraph that begins:

> In the early 1970s Sagan and Lave and Freeberg compared the public health risks of various energy-generating technologies and concluded that in comparison with coal-fired plants, nuclear power offered substantially lower risk to the public's health.

You are aware of literature to that effect, are you not?

A. Yes. Yesterday we talked about our Exhibit 507 where some of the references had made similar comparisons.

Q. In fact, I think the next sentence refers to:

> Hamilton's study from Brookhaven

National Laboratory in Upton, New York, which reinvestigated the issue in 1974 and reported that a modern coal-fired plant still is not as safe as a nuclear power plant.

And Hamilton is one of the authorities that you have referred to in Exhibit 507; correct?

A. Yes, Hamilton is referred to in 507.

Q. You would accept that that's one of the respected authorities in the literature on this kind of risk analysis?

A. He certainly has published quite a lot in this area, yes.

Q. And these experts go on to say:

That for coal underground mining and air pollution dominate both the morbidity and mortality estimates, followed by the hazards of transport. If coal is mined underground and transported by rail, the fuel cycle for mining to combustion is estimated to produce 279 illnesses and injuries, along with 18.1 deaths per gigawatt-year.

And that kind of figure is found in the literature and would be one of the figures that was

reviewed by Ontario Hydro staff in putting together the tables in Exhibit 507 and the corresponding table from the fossil panel?

A. Well, I personally don't know these numbers, but I expect you're right, that this would be the kind of number that was reviewed.

Q. And the authors go on:

> In contrast the nuclear fuel cycle with the uranium mined underground is estimated to produce 17.8 - I think it is - cases of illness and injury and one death per gigawatt-year.

And that figure is not a surprising figure to you?

A. No, we have a comparable number in 507, we could look to see what it is but I would guess it's not very different.

Q. And the authors go on in fairness to say that:

> Mortality and morbidity estimates are somewhat uncertain because agreement is hard to achieve concerning the health effects of particulate and sulphur dioxide emissions from coal-fired plants and the risks to the general population

that result from mishaps at nuclear power plants.

[12:00 p.m.]

And that point about uncertainty was likewise made in your Exhibit 507; correct?

A. Yes.

Q. And part of the reality of this is that comparative risk analysis is a relatively new discipline?

A. I think that is so.

Q. Nonetheless, an extremely helpful one in making choices among energy alternatives?

A. Well, I don't have a sort of expert view of that, but I would say so, yes.

Q. You would agree with that proposition, Mr. Johansen, would you?

MR. JOHANSEN: A. Yes, I would.

Q. Thank you. And the American Medical Association experts go on in the column on the right under the heading Nuclear Power: The Physician and Society:

> The United States requires an adequate supply of electricity to run its business, light its homes and schools, air condition its buildings, preserve its

food, provide satisfactory medical care, and for many other purposes.

And that is an obvious proposition, and it applies just as much to Ontario?

DR. WHILLANS: A. Yes.

Q. And nuclear energy is an option for generating electricity as are coal, oil, gas, water, wind and the sun. Nuclear energy also involves the production of ionizing radiation which can adversely affect humans. Physicians should understand the principles of this means for generating power.

And there I take it that we can assume the authors are referring to the responsibility of scientifically trained people to communicate realities about these kinds of choices to the public?

A. In particular physicians, yes.

Q. But anyone with a scientific and technical background has that obligation; fair?

A. Fair.

Q. Over at the last page on the left-hand column the authors state:

> An additional need that physicians can help address concerns the role of science

and society. All persons includes physicians benefit from flourishing science and technology and suffer from languishing ones.

And you would agree with that as a general proposition?

A. As a general proposition.

Q. And if any other member of the Panel feels differently I would be interested in your dissent, but otherwise I will carry on with Dr. Whillans.

To function optimally, members of a democratic society should have a reasonable understanding of scientific principles and concepts which will help them make decisions about major issues such as nuclear power, chemicals in drinking water, and so forth.

And that is part of the difficulty that nuclear power has faced in competing with other energy technologies; fair?

A. I think that is fair.

Q. And then the experts set out their recommendations, which include the following, which perhaps are trite but some are more controversial:

One, there is a need for electricity.

Adequate capacity for generating electricity is necessary for people's health and the progress of society.

None of you has any problem with that proposition? Dr. Whillans, you can be the spokesman for the time being.

A. Well, I take the need for electricity to be sort of a title for that and adequate capacity is necessary, yes. Certainly.

Q. And you would agree as a health effects expert with the third proposition that: Safety of generating electricity during recent decades in the United States -- generating electricity has become increasingly safe and environmentally benign.

And that would apply in Ontario as well, would it not?

A. I think I would refer the safety aspect to Mr. King.

Q. Well, in terms of health effects you would agree that it has become increasingly benign?

A. I can really only speak for the nuclear aspect, and I think it is becoming increasingly safe, yes.

Q. And not to omit item 2, the authors also recommend emphasis on the conservation and efficient use of energy as something that should

continue and accelerate. And we don't disagree with that, do we?

A. I don't.

Q. And fourth, the authors indicate with regard to safety of nuclear power that generating electricity with nuclear power is acceptably safe in the United States, and you would say the same about Ontario?

A. Yes.

Q. And the further conclusions that power reactors in the United States are designed and constructed for safe operation.

You would agree with that as transmitted to Ontario, or translated to Ontario?

A. Well, I think Mr. King's opinion is more important.

MR. KING: A. That was the statement. That was the theme of my evidence in chief.

Q. Certainly. And the eighth recommendation is that with respect to the role of physicians:

> Physicians should have information available regarding how to treat persons injured by ionizing radiation. They have a broad responsibility to advise the

public and respond to anxieties following a radiation emergency. Also, they should help improve public understanding of the benefits as well as the risks of nuclear power.

And as a scientifically trained person, you would agree with that with respect to your own professional discipline, Dr. Whillans?

DR. WHILLANS: A. I think it is important that people with specialized knowledge try to present a balanced view to the public, yes.

Q. Those are recommendations made by the American Medical Association in 1989?

A. Yes. This is a Council report. The Council on Scientific Affairs of the AMA recommends the following, so yes. The date is 1989.

Q. Would you agree, Dr. Whillans, that in making choices about nuclear power it is important for the utility and for the public not to be influenced unduly by false alarms concerning health effects?

A. I'm not sure exactly what you mean.

Q. Well, you are aware that in the past there have been allegations advanced and alarms raised about certain aspects of nuclear power?

A. Could you give me an example?

Q. The relationship between tritium releases into Lake Ontario and infant mortality in the area?

A. Okay. Well, to take that specific example, an alarm was raised, as you say, but it prompted a scientifically fairly thorough study, which resolved to some extent the question of whether there was an unsuspected risk, and, you know, I think that is a reasonable process.

I don't think there is any reason why people can't responsibly raise an alarm and the proper response is that some sort of a scientifically valid study will address it.

Q. At tab 1 of our Volume 2 we find Interrogatory 9.2.57.

THE REGISTRAR: That now is .41.

THE CHAIRMAN: Thank you.

---EXHIBIT NO. 520.41: Interrogatory 9.2.57.

MR. HAMER: Q. And I haven't attached all of the documents submitted in response to that interrogatory as part of this volume. I have attached only the memorandum to D. McArthur from G. Armitage, and an enclosure to that memorandum being an article called "Who Speaks for Science".

I think the record should indicate that

that is what we have now marked as the exhibit since not all of the documents are included in my excerpt here, if that is acceptable with the Registrar.

And Mr. Armitage, who wrote the memorandum dated December 23rd, 1988, is a colleague of yours, as I understand it, in the health/physics area?

DR. WHILLANS: A. He is the Manager of the Health Physics Services Department within the same division, Health and Safety Division, yes.

Q. And, in fact, you have co-published articles with Mr. Armitage, if I have reviewed the literature correctly?

A. I guess that is true, yes.

Q. And this memorandum represents Mr. Armitage's response to an alarm which had been raised being the one that you have just spoken about?

A. Yes.

Q. It had been suggested that tritium emissions in the late 1970s at Pickering bore some relationship to newborn infant fatalities; is that correct?

A. Yes, that is correct.

Q. And basically what Mr. Armitage's memorandum does is set out at page 3 some gross statistics dealing with newborn death rates in

Pickering and surrounding communities and fatal birth defects in the same area; correct?

A. Yes.

Q. And then at page 5 he refers to statistical testing of that data as having shown no correlation of newborn infant death rates and total tritium emissions or of fatal birth defects and tritium emissions for the Towns of Pickering, Ajax, Whitby, or Oshawa; correct?

A. That is what it says, yes.

Q. And looking at page 6 it appears that Mr. McArthur had raised doubts about Ontario Hydro's ability to measure tritium. That is in the indented paragraph on page 6.

And Mr. Armitage responds with an offer to have blind samples submitted to the Ontario Hydro laboratories for analysis to test your ability to measure tritium; correct?

A. Yes.

Q. Was that offer ever taken up; do you know?

A. Not to my knowledge. I could point out, though, that there are a number of other blind testing intercomparisons in which we participate with respect to tritium measurement, and, you know, this was

sort of an offer beyond those.

Q. All right. And you are confident in your ability to measure tritium?

A. Yes.

Q. In the interrogatory response there was included an article by Dixy Lee Ray which Mr. Armitage had sent out, and have you had an opportunity to review that enclosure to Mr. Armitage's publication?

A. I read it some time ago. I didn't review it again recently.

Q. And we see at the end of that article which is published in, as I understand it, the Health Physicist's Society Newsletter?

A. Newsletter, yes. Not a peer reviewed publication.

Q. I beg your pardon?

A. It's not a peer reviewed publication.

Q. It's a newsletter?

A. It's a newsletter.

Q. And it refers at the end of the article to other false alarms raised by a Dr. Ernest Sternglass on page 12?

A. Yes.

Q. At the bottom of the left-hand column the author states:

Dr. Ernest Sternglass, much quoted by the media on radiation matters, has never published his claims about the effect of low level radiation in a peer reviewed journal. In an article in Esquire magazine published in 1969 Dr. Sternglass predicted that all children in the United States would die as a result of fallout from nuclear tests. Twenty years have passed and unfortunately for his credibility but fortunately for children he was and is wrong.

Had you ever heard of Dr. Sternglass before?

A. I have heard of Dr. Sternglass, yes.

Q. The author goes on:

But his opinions long since dismissed by knowledgeable scientists in his field are still actively sought and quoted by the popular press. Until respected scientists, perhaps through their professional societies or through the National Academy of Science, identify the purveyors of misrepresentation, we have only ourselves to blame for fear, misunderstanding and the rejection of

technology.

And that is not a bad observation, is it?

A. No, I think we have to be careful about just dividing everybody into people who misrepresent and those who have the truth.

Q. Quite.

A. You know, there is clearly a spectrum.

Q. Yes. All right. Now, speaking of dealing with concerns that are raised and responding in a measured and scientific manner, there have been more recent suggestions that there may be a connection between tritium releases and Down's Syndrome in the Pickering vicinity; is that correct?

A. Well, following on what we were talking about a few moments ago, the alarm, as you say, raised by Mr. McArthur, the Atomic Energy Control Board commissioned a study by epidemiologists at Health and Welfare Canada who hold all the data on birth defects in Canada, and they have recently published a report which reviewed the same kinds of health measures, birth defects, for the Pickering area for a much larger period. I think it was from 1971 to about '87 or '88. And this is probably the study to which you are referring.

Q. All right. And that study found, first of all, that the numbers did not support a hypothesis of increased stillbirths, neonatal mortality, or infant mortality in the vicinity of the Pickering station, first of all?

A. Generally, that is true, yes. I can give you the exact quotations, if you want them.

Q. This is a lengthy report. I think you and I are both looking at copies, and I have copies here. I don't know that we need to enter the entire report as an exhibit, Mr. Chairman.

A. Maybe you should just give a reference. It is in the AECB report, INFO-0401, and it is published in October of 1991.

Q. And the authors also did detect an elevation in the birth prevalence of Down's Syndrome in Pickering and Ajax but concluded that the interpretation of that elevated risk must be very cautious.

A. Yes, I think the point here is that they looked at 22 different categories of birth defects and found one that was statistically elevated.

When you do multiple testing like this just because you are setting a criterion which says that something shouldn't occur by chance more than 5

per cent of the time - that is the 95 per cent confidence limit - you expect that one in 20 roughly may occur just by chance.

I think the interpretation they made was that this is the one that did occur. If you are going to focus on any particular birth defect that might be the one you would look at.

But they went beyond that and said: Is there any correlation between the appearance of these in time and tritium releases? And they didn't find such a correlation.

Q. Let me stop you there for a moment. They compared the dates at which tritium releases were elevated with the occurrence of Down's Syndrome?

A. Yes.

Q. And found...?

A. And they found that there was no correlation. Maybe I should say exactly what they say here: There was no consistent pattern.

I am quoting from the document we just referred to:

> There is no consistent pattern between tritium release levels and Down's Syndrome birth prevalences. Chance cannot be ruled out for the association.

And it goes on.

So one I guess what I was trying to say is that if you were going to look at any of the 22, you would look at the one that appeared statistically high, but when you go beyond that and try to ask whether it had any relationship with tritium they couldn't find such a relationship.

Q. The general literature does not disclose a recognized association between Down's Syndrome and low level radiation; is that fair?

A. Well, I think particularly with respect to Down's the literature is mixed. There certainly are published reports which appear to show a relationship, for example, between Down's and the number of x-rays the mothers had during pregnancy. But there are others which do not show that. In the large study I referred to of the survivors the atomic bombings of Japan has no such elevated risk.

I think, as you know, Down's Syndrome is a trisomy. There is one extra chromosome, and these kinds of chromosomal changes have been seen with radiation exposure, but -- so I think that is why there is interest in whether or not there might be a relationship.

But, as I say, the epidemiological

evidence to date is that there is no clear association.

Q. Given the relative levels of radiation from nuclear generating stations with other naturally and artificially occurring sources of low level radiation I take it it would be difficult to sort out, assuming a relationship between radiation and Down's Syndrome, which source was responsible?

A. Well, there are many problems. I mean, there are some strong determinants, factors which influence Down's - for example, mother's age - but what you say is also correct.

[12:20 p.m.]

There is no reason to believe that exposure to tritium is different from exposure due to any other kind of radiation.

Q. Like radon in the house?

A. Well, radon may not be the best example, because radon particularly irradiates the lung, but the other things that I was talking about the cosmic radiation or terrestrial or internal, the doses received as a result of any tritium in the environment are comparable to those.

And so, as I said in the direct evidence, since we are talking about exposures to the most supposed population in this Pickering area which are

only a per cent or so of background, it would be difficult to expect that there would be any relationship with Down's.

Q. Nonetheless, as responsible scientists one continues to monitor that sort of concern as it's raised?

A. Yes. And the agency within Health and Welfare that I mentioned maintains a national congenital anomalies surveillance system just for that purpose, not with respect to tritium only, but for any other reason, and so there is sort of ongoing analysis all the time.

DR. CONNELL: Excuse me, could I ask, do we have on the record any description of the releases at Pickering that are cited in this document, the timing and the magnitude?

I take it we are talking about atmospheric releases rather than --

DR. WHILLANS: Or to water.

DR. CONNELL: Are they differentiated in the study?

DR. WHILLANS: The report that we are talking about covers a period going back to the early 70s. I think the only evidence we have put in, unless it is through some of the interrogatories, and I am not

clear on this, refers to perhaps the past five years. Perhaps we should check that.

DR. CONNELL: I think it would be helpful just to have a brief summary, because I am sure it will come up again.

MR. HAMER: Mr. Chairman, I am happy to file a report as a whole or as well it has sort of an executive summary on the front. I think it might be just as well to file the whole thing. It has some statistical tables in it as well. I can't pretend that I appreciate everything that's in it.

THE CHAIRMAN: I am not sure that's the question that Dr. Connell is asking. I think he would like some kind of an analysis of what the tritium output is both through water and air.

DR. WHILLANS: We have filed some of that information and I will check to see whether it's complete.

DR. CONNELL: If it's in the document I would be happy with that.

THE CHAIRMAN: It may be in the document.

The document has been referred to several times, so perhaps we should put it in.

DR. CONNELL: May I also follow up your observation that tritium is like any other radiation

source. I wonder if you could clarify that tritium is a very soft form of radiation, is it not?

DR. WHILLANS: Well, I guess that was perhaps an over-generalization.

The tritium exposures that we are talking about as a result of releases from Pickering are released actually in the form -- well, entirely in the form of tritiated water from Pickering. And the exposures occur either from a member of the population drinking water that contains some of the tritium, from inhaling some of the air-contained tritium, and also from the deposition of that tritiated water onto the soil and being taken up in food which may be eaten or in other things.

Tritiated water exposures in the body, and these are the kind of exposures we see in the stations in the Pickering workers, are as a result of intakes of tritiated water. The exposures that result from that, because tritiated water or any other kind of water distributes uniformally throughout the soft tissues, the exposures from that are not different from any other kind of sort of large whole body exposure.

Now, in the environment there is an additional concern that some of the tritium that is ingested may be in the form of an organic-bound

tritium, and there have been numerous studies looking at that form of exposure, studies looking at how much of the tritium in a typical environment is in that form, and also some studies in an experimental setting, looking at how that kind of intake compares with the tritiated water intake.

I guess as I said, I generalized that they were not different. They are not very different. The amount of tritium intake that is as organic-bound tritium is a small fraction in the critical groups that we talk about, and the dosimetry for that is believed to be within a factor of two or so.

Getting away from sort of the environment around Pickering, it's certainly true that an intake of tritiated thymidine or something like that would be very different, but those aren't the kinds of exposures we are talking about here.

DR. CONNELL: I would like to just establish two things: One is that tritium that is not ingested or inhaled, apart from the deposition question, is essentially harmless. That is, if this water pitcher were full of tritiated water, as long as it had a secure stopper in it, it would be harmless.

DR. WHILLANS: Certainly. Tritium is entirely an internal hazard. I mean, it's a pure beta

emitter with a very soft beta, it would pass through the container as you say.

DR. CONNELL: In contrast to I131.

DR. WHILLANS: Or the noble gases we were talking about, yes. That's right.

DR. CONNELL: And the other point is, you may not have this information but, if we are talking just about tritiated water and ingestion of it, a factor to consider here is the biological half life which is quite distinct from the isotopic half life.

DR. WHILLANS: Yes. Well, the isotopic half life is 12 plus years. The turnover in a so-called reference man -- the ICRP also has a set of recommendations about typical physiological parameters and they often refer to a reference man who is sort of a typical northern European/North American male. The turnover time for tritiated water, so the biological half time that you are referring to is about 10 days. It's very different from the 12 years. And for children it's somewhat different and for women it's slightly different, but in that range.

DR. CONNELL: And very different from strontium which can get absorbed in calcified tissues.

DR. WHILLANS: That's right. Other kinds of internal hazards, carbon, for example, or strontium,

are incorporated by the normal metabolic processes into special tissues. As you say, strontium goes to bone because its like calcium, and that's not true of tritiated water.

Tritium is an isotope of hydrogen so it is incorporated to a small degree as the body makes chemicals that contain hydrogen, but that's also included in the dose factors that we use. It's a very small percentage, about a per cent or so.

But apart from that, it behaves just like water so it turns over fairly quickly.

DR. CONNELL: Thank you.

MR. B. CAMPBELL: Mr. Chairman, Dr. Connell, if I could point out, there is tritium release information contained in the report which I guess should be given the next exhibit number.

THE REGISTRAR: That will be No. 554.

---EXHIBIT NO. 554: Document entitled Tritium Releases from the Pickering Nuclear Generating Station and Birth Defects and Infant Mortality in Nearby Communities 1971-1988.

MR. B. CAMPBELL: It's referred to in a variety of places; for instance, on page 30 and 31 there is a full history to the end of '88, which I gather is the period over which data was analyzed of tritium releases both airborne and water. This can all

be expressed different ways. And there were various later diagrams commencing at page F3, for instance, dealing with tritium observations at various monitoring stations maintained by Health and Welfare Canada, some correlation of that data, page F8 and F9.

Perhaps it would be convenient, Dr. Connell, if that information is sufficient for your purposes, once you have had a chance to look at it, then that could serve the purpose. Otherwise, if there is additional information that you require, if you could let us know then we will make sure that it gets produced.

DR. CONNELL: Thank you.

MR. HAMER: Q. Dr. Whillans, I believe you have also had an opportunity to look at an excerpt from the Hinkley Point inquiry report, is that so?

DR. WHILLANS: A. Yes, that's true.

Q. And this is found in Volume 3 of our book, tab 7. I will be asking the witness a number of questions about this document and perhaps it might be given an exhibit number.

THE REGISTRAR: This one is 555.

---EXHIBIT NO. 555: Document entitled "Hinkley Point Public Inquiries", A Report by Michael Barnes, Q.C.

MR. HAMER: Q. It's an excerpt from

Volume 5 of the Hinkley Point Public Inquiries Report being chapter 41 dealing with leukaemia clusters.

DR. WHILLANS: A. I looked particularly the introduction and the conclusions and in some parts of sections B and C.

Q. I put this to you simply as a well written lay person's description of the literature and some of the arguments that have been advanced concerning leukaemia clusters in the area of nuclear installations. And without committing to you detail, may I ask if you too found it a helpful and instructive discussion of that issue, based on your knowledge of the actual scientific literature?

A. That is a fairly large generalization.

Yes, I found it helpful. I think one point I would make is that this was published in 1990, and the whole issue of leukaemia clustering is very much sort of an active area.

Q. Yes.

A. I referred to some of the initial studies in my direct evidence around Sellafield, since that time there have been many more; in fact, there was a workshop on clustering at the AECB just two weeks ago and some people referred to this in report presented

new evidence.

So yes, I found it interesting particularly because it was from a British perspective. I am not sure it's entirely up-to-date.

Q. And the U.K. was where the leukaemia cluster theory or concern originated, was it not?

A. Well, the idea of leukaemia clustering goes back much further than 1983. Even in the 50s there were leukaemia clusters found in the U.S. and there have been investigations of what they causes may be and so forth, but nothing to do with radiation.

Q. It goes back before nuclear power, does it not?

A. Oh, yes.

Q. And in fact, that's some of the evidence that was before Mr. Barnes in the Hinkley Point Inquiry; correct?

A. Yes.

Q. And it has been found that leukaemia clusters occur in areas now where there is no nuclear installation?

A. That's true.

Q. And it has been found that they occur in the area of some nuclear installations in the Untied Kingdom but not the vicinity of other nuclear

installations in that country?

A. Yes.

Q. In Canada investigations have been done on that topic as well, in Ontario?

A. In Ontario, yes.

Q. And while the statistics may move up and down depending on the station and the time of the study and so forth, in general there has been no established connection discovered between the presence of a nuclear station and leukaemia clusters in a causal sense; is that fair?

A. I think you are saying two things there.

Q. Probably.

A. I referred to this in my direct evidence, I believe it's also been sent out, the report that you are referring to has been sent out, in response to an interrogatory.

Whenever you are looking at a study like this you have a number of cases and you compare that with a number of expected cases or perhaps with a control group, you come up with some kind of a ratio such as we were talking earlier in the occupational mortality studies, and you can find out whether you have more or less and you have some kind of confidence

limit which is based on the sort of sampling statistics associated those numbers.

So one thing that you have just said, I think, is that in the studies that were done in Ontario around five different sites, none of them was statistically greater than one.

The second question though is, even if you do find a mortality ratio, say, or relative risk which is statistically greater than one, that doesn't necessarily mean that it is a causal relationship. And there are a number of criteria that epidemiologists follow in trying to go from a statistical association to a causal relationship, and these are biological plausibility, consistency as you say amongst other similar situations, things which try to control for confounders which are beyond what the single study can detect.

Q. What is a confounder?

A. A confounder I think technically is a cause of a disease which is associated with an exposure. And the practical implication of that is that if aren't aware of the confounder you may assume that the exposure caused the disease when in fact it was just because it was related to what really did cause the disease.

There is a lots of examples that are used, but the one I remember is there is a very strong association between the number of drownings in a given month and the sales of ice cream, but that doesn't suggest that ice cream causes drowning. There is a common association with warm temperatures, I think.

Q. And to put things another way, am I correct in recalling that in the Ontario studies it was found that around my client's nuclear installations at Chalk River it appeared that children were healthier than they are elsewhere, and it wouldn't be reasonable for my clients to claim that it made children healthy to live near their installations because of the relatively small differences in the numbers that turn up there. Is that a fair statement?

A. Healthier in the sense that the incidence of childhood leukaemia was less. In my evidence I believe I quoted numbers between something like .3 and .7, depending on just exactly how the analysis was done.

Certainly, the relative risk of a child in that part of the province suffering leukaemia was less than in the province as a whole.

Q. Statistically?

A. Yes.

Q. Yes. Well, I think that discussion has been helpful.

I would like to take you to a few specific passages in the Hinkley Point excerpt. At page 1500, paragraph 41.5, and I take it you would agree with the observation that fortunately leukaemia is a comparatively rare disease, and Mr. Barnes indicates that only 2 per cent of all cancer registrations in the England and Wales were attributable to leukaemia.

A. Yes.

Q. And that's a familiar kind of ratio?

A. Yes. We have been talking about childhood leukaemia, and I think he makes the distinction someplace in here, but there is two different periods in live in which leukaemia becomes more prominent, one is the early ages up to maybe age 14, and then late in life it becomes much more common, and I think this refers to the whole set.

Q. One of the things that was found was that one did not find clusters of all leukaemias as opposed to merely childhood leukaemias in the area of nuclear installations; isn't that right, and that was significant?

A. This is in this paragraph, 41?

Q. No, I'm sorry. I am going broader now.

A. Sorry, could you repeat it, please?

Q. Yes. You raised the point that this paragraph is speaking about all leukaemias.

A. I think so, yes.

Q. And I was jumping forward to observations elsewhere that have made in this report and elsewhere that one does not detect clusters of all leukaemias as opposed to childhood leukaemias in association with nuclear stations, and that is a significant finding; is that fair?

A. Well, I am not sure whether that's actually been looked for.

Q. I'm sorry?

A. I am not sure whether that's actually been looked for.

Certainly the Ontario studies looked only with a restricted age range up to age 14 at childhood leukaemias. Studies around Sellafield looked only at childhood leukaemias. Some of the other studies in the U.K. which looked at all cancers looked up to about age 24.

I guess what I am saying is I am not aware that there have actually been studies around

these facilities, for example, that looked at leukaemias late in life. That's not to say it hasn't happened.

Q. So, I may have got it wrong.

What Mr. Barnes did look at was whether or not there was an increased incidence of all cancers in the vicinity of nuclear stations, and if there was a relationship between nuclear stations it and leukaemia one would also expect to see an elevation in all cancers; is that fair?

A. Mr. Barnes was looking from the point of the view of the area of Somerset around Hinkley Point and he reviewed evidence which was much wider. So yes, he has looked at the incidence of all cancers around nuclear stations, yes.

Q. And the hypothesis being that leukaemias cluster around nuclear stations because of the nuclear station. If that were correct, one would expect to see cancers clustering around nuclear stations due to the same causal relationship; fair?

[12:40 p.m.]

A. Well, I think it is dangerous to treat all cancers as a single disease.

Q. All right.

A. You know, I am not an expert in this

area particularly, but certainly I think it is believed that different mechanisms may be involved with different kinds of cancer, so that it is possible that some are related to radiation exposures and others aren't.

For example, in the studies of the survivors of bombings in Japan some kinds of cancer seem not to be at all related to radiation dose and others do, so I am not sure that we should just consider them all together.

Q. But some cancers are specifically associated with radiation exposure, not just leukaemia?

A. Yes, that's true.

Q. All right. And if there was a causal connection between nuclear stations and leukaemias so that clusters occurred in those vicinities one would also expect to see a cluster of the other kinds of cancers that are associated with radiation exposure?

A. Well, whether you would expect to see it or not is a difficult question.

I don't know of any kind of cancer for which it is thought that radiation is the only cause. In fact, it is probably a very minor cause for almost every kind of cancer, and so in order to be able to see a cluster you have to be able to pick out cases that

are caused by that particular exposure from all the others.

Some of these other cancers, for instance breast cancer, are so common that I would think that you wouldn't see a cluster of cases caused by association with radiation just because there are so many other cancers that are caused by other things - breast cancers, for example.

Q. Well, if we go --

A. Maybe I am missing your point.

Q. I think it is because you understand it better than I do.

If we go to page 1505 of the report, paragraph 41.12, you see Mr. Barnes saying:

> I propose to approach the above task - and that is considering the causal connection between leukaemia and nuclear stations, as to whether there is one, that task - by considering first the evidence on the incidence of cancer generally, i.e. all forms and in all age groups, and of leukaemia generally around nuclear installations.

A. Right.

Q. And that was a legitimate and valid

approach, was it not?

A. Well, this is how he is focusing in on the things that I think he was asked to check initially. He is starting from a point of view of looking at all cancers generally, yes.

Q. And then he says three or four lines down:

> I will then go on to consider the evidence on the specific question of clusters of childhood leukaemia in the vicinity of nuclear installations.

A. Yes.

Q. Right. And then he has listed in an earlier paragraph the various United Kingdom studies that he reviewed, and he indicates:

> Having referred to the studies I will examine and explain the evidence and the contentions on what is the crucial issue; namely, whether there is any established causal link between discharges of radioactive materials from nuclear plants or some other aspect of their operation and the raised incidence of leukaemia among children living in their vicinity.

And then in the body of his report he

goes through a great number of the studies which have been conducted in the United Kingdom and elsewhere and the evidence which was placed before him.

A. Yes.

Q. That is correct?

A. Yes.

Q. And then at page 1547 under the heading "The Evidence on Causation", he says:

> It seems incontrovertible that childhood leukaemia clusters do occur, both in geographical areas and in time. Some of these clusters have occurred in the vicinity of some nuclear stations.

And, parenthetically, you would agree that some clusters occur elsewhere, far away from nuclear stations?

A. Yes, I think that is so.

Q. Yes. And he says:

> It seems unlikely the clusters occur solely by the operation of chance.

And you would agree with that?

A. Yes.

Q. And he says:

> It is plainly a matter of high practical importance, both from a

scientific and a humanitarian point of view, to discover the reason or reasons for the clustering. It seems to me that it is greatly in the interest of the nuclear industry that an authoritative explanation should be found.

And you would agree with that, and you would have to acknowledge that as of today we don't have all the answers on the causes of any of these clusters, whether near or far from nuclear stations?

A. I think that is true, yes.

Q. And then at page 1558 at paragraph 41.112 Mr. Barnes refers to the vigour of the debate on leukaemia causation, and, not to read it all, a draft paper had been submitted to the inquiries, and then a newspaper report was put into evidence indicating that the draft paper showed a relative increase in the incidence of childhood leukaemia in certain areas of the country and that that was related to fallout, and then some other authorities wrote to the newspaper saying that this was seriously misleading.

And then, on page 1559, the work of Bentham and Haynes - that is the draft paper, was almost immediately seized upon by Dr. Robin Russell Jones, Chairman of the Pollution Advisory Committee of

Friends of the Earth, who wrote to The Lancet, concluding his letter with the following paragraph:

> These new data, i.e. the findings of Bentham and Haynes - that is the unpublished draft paper - remove the remaining scientific obstacle to accepting that radioactive discharges from the two reprocessing facilities in the U.K. are responsible for the excess cases of leukaemia and lymphoma in the surrounding population.

And you wouldn't agree with that conclusion by that author, Dr. Robin Russell Jones, would you?

A. Well, without having seen the unpublished draft report I can't be specific, but no, I don't think so.

You also skipped a statement that Richard Doll and Sarah Darby had responded to the letter and made certain comments about it, and these are people who are very well known for their studies of causes of cancer, epidemiologists who are known for that, and I would take their opinion seriously.

Q. Well, they were the ones who wrote saying it was seriously misleading to say that a

connection had been established between bomb fallout and childhood leukaemia?

A. That is right.

Q. Dr. Robin Russell Jones concludes:

> The only problem now is political, the recognition that British children are among the first victims of an independent nuclear deterrent.

And I take it that you would agree that that is not really the way in which to address that concern at this time, as a political concern?

A. No, I don't think that is helpful.

Q. Not to be pejorative about the term 'political'.

And then Mr. Barnes makes the point that we have been making for some time:

> This extract may give some flavour of the way in which any new and tentative advance in scientific research is sometimes used.

And you would agree with that on that controversy, wouldn't you?

A. It would seem so, yes.

Q. I believe you referred in chief to the Kinlen hypothesis relating to leukaemia clusters;

is that correct?

A. I did, yes.

Q. And Kinlen, as I understand it, has postulated that where a relatively isolated population comes into contact with an incoming group of people from a different part of the country there may be some infective link between the arrival the newcomers and the appearance of leukaemia clusters?

Have I got that roughly correct?

A. Yes, I think that is roughly right. Kinlen's hypothesis, I think we have to still regard as a hypothesis--

Q. Sure.

A. --was in response to the Sellafield situation I described where there was a statistically, very significant excess risk of childhood leukaemia in a small village.

And the idea that leukaemia was associated with infective agents is not new. Some of those studies we talked about in the 50s in the U.S. were looking for just that, and in other animals, mice for example, viruses cause leukaemia. So it is not a new idea.

What he did was to look at other areas away from nuclear facilities, he specifically looked at

a new town in Scotland, known as Glenrothes, and found that using the same sort of methodology there was a statistically significant increase in that town and there was no association with radiation.

Q. But there had been population mix?

A. There had been. Because it was a new town, a sort of a town that had been set up in a planned way in a rural community there was a large influx of people into that community.

Q. And Kinlen doesn't claim that he has proved a causal link between population mixing and leukaemia, but it is fair to say that he strongly suspects one, based on his --

A. I think one of your tabs here has a very recent publication of his where he looks at another kind of population movement, the return of soldiers, I think, after the war into communities and how that affected leukaemia rates.

Q. Well, if we can keep our place in the Hinkley Point Report and go back to tab 5 in the same book, that is the report to which you refer by Kinlen, et al, entitled Childhood leukaemia and Poliomyelitis in Relation to Military Encampments?

A. Right.

MR. HAMER: Mr. Chairman, I wonder if

that might be given an exhibit number as well.

THE REGISTRAR: 556.

THE CHAIRMAN: Thank you.

---EXHIBIT NO. 556: Hinkley Point Report, tab 5, report by Kinlen, et al, entitled Childhood leukaemia and Poliomyelitis in Relation to Military Encampments.

MR. HAMER: Q. And if I might correct you for once, he wasn't talking about returning servicemen in that study; he was talking about the high level of national service and people being off in army camps in remote areas in England, was he not?

DR. WHILLANS: A. I think you are correct, yes.

Q. I'm sorry, the copy came off a fax and then has been mutilated in the binding process, but if we go to the second last two facing pages we see a table on the right-hand and on the left-hand side, and in the right-hand column on the left-hand page there is a line I would just like to have on the record, and it is not very legible but towards the bottom, almost in the centre of that column from top to bottom you see the sentence:

> The associated excesses of childhood leukaemia are therefore highly relevant to the hypothesis based on infection

which postulates that an appreciable increase in the level of new social contacts in a community can increase the incidence of leukaemia.

A. No, I can't find that.

Q. You can't read it?

A. Where is this, I'm sorry?

Q. I'm sorry. I am looking at two pages, the number of which have been obliterated. If you look at mine and you see there is tables facing each other?

A. Okay. Yes.

Q. And I have highlighted in green in the middle column the passage which I just read.

THE CHAIRMAN: Let me see that?

MR. HAMER: Sorry.

DR. WHILLANS: I'm sorry, how did that passage start, the first few words?

MR. HAMER: Q. It is at the end of the middle paragraph in that right-hand column, and if you look underneath the spiral binding you will see "The associated excesses of childhood leukaemia...", about six lines from the bottom of that paragraph?

I do apologize for this.

DR. WHILLANS: A. I should get out my

own copy. Oh, yes. Here it is.

Q. The associated excesses of childhood leukaemia are therefore highly relevant to the hypothesis based on infection which postulates that an appreciable increase in the level of new social contacts in a community can increase the incidence of leukaemia.

And that is his basic point?

A. Yes. He has a number of publications, I think some of them are referred to here, which explore basically the same mechanism.

Q. And then, just to refer to one other passage on the back page, he summarizes at the end:

The findings support the hypothesis that prompted this study that the presence of large numbers of servicemen, particularly in rural districts, was conducive to an increase in the incidence of childhood leukaemia. They also point to an infection transmitted among adults as implied by a recent study of the effects of the population mixing associated with increases in commuting.

And that was another study carried out by others, as I

understand it; is that correct?

Oh, no. It was Kinlen and others who studied the effects of --

A. I really can't read this copy at all, certainly not down to the point of reference.

Q. All right. And he concludes, saying:

The increase was greatest in children under one year, which suggests intrauterine infection with transmission from the servicemen population presumably directly or indirectly by the husband.

A. That is what it says.

Q. And that is his hypothesis?

A. That's right.

Q. All right.

DR. CONNELL: Do you have the journal and the date?

MR. HAMER: Yes, Dr. Connell. It is the British Medical Journal, Volume 303, November 30th, 1991.

DR. CONNELL: Thank you.

DR. WHILLANS: I think the page number is 1362?

THE CHAIRMAN: You mean the first page?

DR. WHILLANS: Pardon me? I was actually

asking -- the first page is 1362?

MR. HAMER: Q. Ms. Findlay tells me it is 1357 to -62.

I will endeavour to obtain a better copy of that and file it, Mr. Chairman. I think some of the footnotes should be made more legible.

And then going back to Mr. Barnes' report at the back of my Volume 3, tab 7, at page 1567, paragraph 41.128 at the bottom of that page, Mr. Barnes notes that:

> Dr. Kinlen has pointed out that work has been done on the occurrence on clusters of cancer cases among children in areas away from the vicinity of nuclear installations. As long as ago as the end of the 19th century a pair of leukaemia case occurring together was a cause for comment. In 1963 it was reported that in Niles, Illinois, a small suburb of Chicago with a population of 20,000, eight cases of leukaemia occurred in the period 1957 to 1960.

And you are familiar with that occurrence?

DR. WHILLANS: A. Yes, I am aware of the study. Yes.

Q. And then about 10 lines down, page 1568:

> Over the next few years a number of similar clusters were reported from a variety of locations, mostly in the United States.

And about 10 lines further on, referring to studies by Craft, Openshaw and Birch in the United Kingdom, Mr. Barnes writes about 10 lines above the next subtitle:

> Many small areas of the Northern Region - that is, of the United Kingdom - could be claimed to have an excess rate of childhood cancer. This study is of some importance in establishing that clusters of childhood cancers are by no means confined to the localities which contain nuclear installations. I mentioned earlier that in North Wales an excess of leukaemia cases was discovered, but in elderly people not in children.

And you are familiar with that work as well?

A. The study in North Wales?

Q. Yes.

A. No, I'm not actually. That is contrary to the point I was making earlier on, that I

wasn't aware of studies that looked at excesses in older people.

Q. All right. But are you familiar with Craft, Openshaw and Birch's work?

A. Yes.

Q. And then if we could go to Mr. Barnes' conclusion, which begins on page 1573, and I will start in at 1574, paragraph 41.142.

[1:00 p.m.]

He says in the second sentence:

> It is plainly impossible to conclude with complete certainty that a certain physical factor such as discharges of radioactive material does or does not cause a raised incidence of a certain disease such as leukaemia among those living in a particular locality when the full causes of the disease and the exact mechanism by which it is induced are not known.

You would agree with that?

DR. WHILLANS: A. Yes.

Q. And you would agree with the next sentence:

> Much of the evidence involves a

consideration of statistics, the compilation of which sometimes involves uncertainties since no complete and accurate past record exists of the incidence of or deaths from childhood leukaemia within limited areas or nationally.

A. Yes, it sometimes involves that.

Q. And about 10 lines from the bottom of the page Mr. Barnes says:

> Finally, discussion is not helped by the way in which advances in knowledge are sometimes treated as providing ammunition against the nuclear industry rather than as steps towards arriving at the truth. Valuable scientific papers are distorted beyond recognition in the way in which they are reported.

And you are familiar with that process, I am sure.

A. I have seen that happen, yes.

Q. Over on the next page Mr. Barnes refers to a local councillor who claimed to know of a cluster of leukaemia cases in an of Weston-super-Mare.

> It emerged that when he talked to the

local health authority they had concluded there was no excess of cases and that he had no idea of the expected number of the cases for the area and time by reference to which it could be judged whether the actual cases were in truth an excess number. Such anecdotes are worse than useless.

And one has to get away from the anecdotal in this kind of issue, does one not?

A. Yes. When I read this last night I thought he had been a little hard on the local councillor, because often people who are not trained in scientific methods for judging whether something is real or not can nevertheless notice something which brings an important issue up.

I think the important thing is the response to that kind of an alarm, as you said earlier.

Q. And Mr. Barnes himself says elsewhere in his report that, indeed, there is nothing wrong with raising concerns like this; the issue is how does one deal with them.

A. I agree.

Q. And that has to be on a rationale basis.

A. I agree.

Q. And indeed, in the next paragraph Mr. Barnes says:

> Any person unaquainted with the full range of technical evidence available on ionizing radiation and leukaemia can be forgiven for adopting a short and seemingly logical train of thought.

And that's the train of thought that you just described.

A. You are asking me?

Q. Let's go on then. He says:

> The process of reasoning is that ionizing radiation can cause leukaemia and other cancers. Concentrations of childhood leukaemia have been discovered near some nuclear installations which discharge the radioactive material which emits ionizing radiation and therefore it is the radioactive discharges from the installations which have caused the excess cases of leukaemia.

What you are saying is that people can be forgiven that logic but it ain't necessarily so?

A. Well, I think that's right. The case

I just described was Sellafield, and that's exactly what happened. It was a television documentary that have identified the cluster, a reputable group, the National Radiological Protection Board did a very careful assessment of what the releases from the station had been over that whole period and decided that it was very, very unlikely that they could account for it -- if they did account for these cases, then our estimates of risk were wrong by factors of 100, and there was other reasons to believe that wasn't true. And that kind of approach led them to look for other cases of a cluster.

MR. HAMER: Mr. Chairman, I had thought I might get through this report before lunch, but I would like to spend a little more time on it.

THE CHAIRMAN: All right. We are adjourned until 2:30.

THE REGISTRAR: Please come to order. This hearing is adjourned until 2:30.

---Luncheon recess at 1:05 p.m.

---On commencing at 2:30 p.m.

THE REGISTRAR: Please come to order. This hearing is again in session. Please be seated.

THE CHAIRMAN: Mr. Hamer?

MR. HAMER: Thank you, Mr. Chairman.

Q. Dr. Whillans, we were in Hinkley Point at tab 7 of Volume 3, and we were starting to look at Mr. Barnes' conclusions at page 1575. And towards the bottom of page 1575, about five lines up Mr. Barnes carries on with the simple logic which would lead to an association between nuclear stations and leukaemia clusters, and says:

> There are a number of solid reasons for concluding that the excess childhood leukaemias or leukaemia clusters ascertained to exist near some nuclear plans are most unlikely to have been caused by the radioactive discharges from the plants.

May I take it that you would agree that that is so, there are a number of solid reasons for that conclusion?

DR. WHILLANS: A. Yes.

Q. And we can see some of the solid reasons on the next page at the top of 1576. Mr. Barnes says:

> The primary reason for rejecting a causative link between plant discharges and excess childhood leukaemias is that such a link appears impossible on the

basis of all generally accepted knowledge of radiation dosimetry and risk coefficients.

And you would agree that that is a prime reason for reaching that conclusion?

A. Certainly in the case of Sellafield where a very careful study was done, it's a good estimate of what the discharges were. And as I said, the exposures resulting from those discharges and the risk coefficients which we believe to be true cannot account for the leukaemias, yes. I am not sure that that level of detail has been done in every case.

Q. All right. And his second primary reason is at page 1577, No. 2 in brackets, and he refers to the reasoning advanced by Darby and Doll concerning the effect of weapons testing fall-out which systems seems compelling to him.

And as I understand it, reading a little lower down in that paragraph:

> If low doses of ionizing radiation from such radionuclides caused childhood leukaemia in some way not understood, whether through irradiation of the parents or of the fetus or of the young child, the same causative effect should

have been observed in the irradiation from weapon test fall-out.

You would agree with that logic, may I take it?

A. I am not really familiar with how much dosemetric studying there has been of weapon test fall-out.

I certainly accept as I said that Darby and Doll are people who have looked at this subject thoroughly and I would treat their conclusions seriously.

There has been a recent paper which suggests that alpha emitters may or may not have a different role in radiation induction of disease, and this is just something that's occurring. So I think it is an area where there is still room for research.

Q. Would you agree with his third primary reason on the same page, No. 3:

> Levels of doses from natural radiation are generally much higher than those created by plant discharges.

We have been talking about that fact yesterday and today; correct?

A. That's certainly true for our plants and it's generally true, yes.

Q. And he says two lines on:

If the plant discharges do cause excess childhood leukaemias then, due to the effect of natural radiation, much higher levels of leukaemia should exist generally than are in fact found.

And again, can we take it that that logic is fair as well?

A. It's a generalization. I think it is fair. In my discussion with Dr. Connell this morning we were talking about whether different sources of radiation might have different effects. And while it's true that the levels are much higher from natural radiation, the specific kind of exposures aren't identical.

So I think it is generally true but it doesn't cover everything.

Q. But it is a valid consideration in coming to a conclusion--

A. Yes.

Q. --as to whether these leukaemia clusters are caused by nuclear stations?

A. Yes.

Q. And you would accept, I take it, from your earlier evidence the fourth reason he lists at the

bottom of that page:

> If plant discharges cause excess leukaemias there should be an ascertainable relationship between the level of the discharges and the number of excess leukaemias caused.

And he notes that nuclear installations very enormously in the level of their discharges and the resultant doses to the population, and then says: No such relationship has been established.

A. Again, I generally agree.

My only reservation has to do with the fact that leukaemia particularly is a rare disease, the numbers in most of these studies small so that the uncertainties are significant. But it's true, a number of studies have been carried out and none of them has detected anything.

Q. And then the fifth reason on the next page we have been been over already and that has to do with the fact that ones finds leukaemia clusters where there is no nuclear station, just as one finds them where -- sometimes one finds them where there is a nuclear station.

A. That's true.

Q. And item of I think we went over

already in our general discussion.

If radiation from radioactive materials discharged from the plants did cause excess leukaemias, it might also be expected to cause excesses of cancers in children. There is no general evidence of such other excess cancers.

Again that is a valid consideration in coming to the conclusion?

A. We talked about the differences between cancers earlier. But certainly the second statement is true, in any of these studies where leukaemia has been found in excess, they have not found the same with other cancers.

Q. And in item 7 it is a valid consideration that:

If radiation did induce leukaemia in children in some unexplained way and to an unexplained extent, these features would presumably have been present and operated on the child survivors of Hiroshima and Nagasaki. No excess number of child leukaemias is recorded in this group beyond that which would be expected from the application of conventional risk

factors.

Again, that is a valid consideration in coming to this conclusion?

A. I am thinking about the second sentence. There is no excess in the child age group beyond that which would be expected. But the conventional risk factors are derived from the whole population which includes that group. So I guess to the extent that he is not saying there is a large difference between childhood leukaemias and others, I agree, and the rest of it as well.

Q. Another way of putting what he has put there in 7 is that the conventional risk factors are valid; is that correct?

A. Well, conventional risk factors are what we use.

In what sense would you mean valid?

Q. Appropriate to use.

A. Well, they are the only numbers we have, the conventional risk factors, so there is not really anything to validate them against.

They are a synthesis of that study and a number of other studies and they are consistent, and I guess in that sense you would say they are valid.

Q. And then 8, Mr. Barnes refers to the

Kinlen hypothesis which we discussed this morning concerning population mixing, and says, at the top of page 1579:

> This hypothesis obviously cannot yet be claimed to have been proved correct, and no it doubt suffers from its own difficulties. Nonetheless, it represents an alternative suggestion which, on the face of it, at least merits examination alongside any radiation link theory.

And you would agree with that?

A. I think many experts in the area would think that it is worth further examination.

DR. CONNELL: I would like to clarify a point about point 3, the natural radiation. Perhaps Dr. Whillans could just give us a little more elaboration.

I take it in the reference to alpha radiation he is probably referring mainly to radon.

DR. WHILLANS: Radon is one, but I mentioned some of the internal sources where the lead polonium -- the uranium series has a number of alpha emitters and they are found in the diet to some extent.

DR. CONNELL: Potassium 40 is not an alpha emitter.

DR. WHILLANS: No, it isn't. It's the actonides generally, the uranium polonium series, thorium, a number of the large heavy elements emit alpha particles.

DR. CONNELL: And the neutrons would tend to go with the alphas, would they?

DR. WHILLANS: I was puzzled when I read this first.

My understanding is there is not very much neutron exposure in the environment, and I really can't help you very much more about that.

DR. CONNELL: So radon decay does not emit a neutron.

DR. WHILLANS: No.

I have a very detailed reference, a report from the NCRP in the U.S., the National Council on Radiation Protection, and it has ever source that's known up to 1987. I could certainly check that if you like for more information.

DR. CONNELL: Thank you.

On point 8 we probably haven't got an authority on viral carcinogenesis, but does anybody on the panel know whether there are any well established cases of viral origin of human cancer?

I recall a while back Burkett's lymphoma

was suspected, but are there any that you know of that are well validated. Apart from the oncogenes of course.

DR. WHILLANS: To my knowledge there hasn't been a proof that's true in humans. As I mentioned, it's certainly true in mice. And there is circumstantial evidence I guess to the extent that there is associations between things like Burkett's lymphoma virus and --

THE CHAIRMAN: I'm sorry, I didn't quite hear that.

DR. WHILLANS: There are associations between some viral viruses and tumors, but I don't think, in my opinion, that it is really considered proof that it's a cause. But this isn't an area where I really would consider myself expert.

DR. CONNELL: Thank you.

MR. HAMER: Q. Flipping over to page 1582, Mr. Barnes sets out his five principal conclusions having listed the reasons for arriving at the basic conclusion. Item 1:

> There is no evidence of a generally raised incidence of cancer around nuclear installations in this country.

And would you accept that that is correct

for Ontario as well?

DR. WHILLANS: A. Yes, that's certainly true for Ontario.

Q. Item 2:

There is no evidence of a generally raised incidence of adult leukaemia around nuclear installations in this country.

Would you accept that as true for Ontario as well?

A. That's true for Ontario, yes.

[2:45 p.m.]

Q. I don't mean to restrict it to Ontario, but I mean to bring it over to Ontario.

A. That's right. I haven't looked through all of this, but I am not aware of probably all the studies that have been done in the U.K. I think it is generally true, and it is certainly true for Ontario, to my knowledge.

Q. And item 3:

There is evidence of a raised incidence of childhood leukaemia around some nuclear installations, although the exact incidence or clustering in time and space follows no clear pattern.

And you would agree with that from what you know of Ontario and of the literature, would you not?

A. Well, there is no evidence of a raised incidence of childhood leukaemia in Ontario.

Q. At all.

A. At all. And it is true, the clusters that have been reported around some installations in U.K. have different patterns, yes.

Q. And fourthly, he concludes:

> On the evidence and on the present state of knowledge it is possible, but most improbable, that there is some causal link between the discharges of radioactive materials from the installations or some other aspect of their operation and a generally increased risk of leukaemia among children living near the installations.

And would you accept that as being a valid conclusion to come to on the basis of the literature and the evidence which is available to date?

A. Yes. And as we have said, it is particularly true for Ontario where there is no raised incidence. It is also true in U.K.

Q. Then the fifth --

THE CHAIRMAN: Assuming the evidence, there is no such evidence in Ontario, assuming that to be so, then conclusion four doesn't have any applicability to Ontario?

DR. WHILLANS: That's right.

MR. HAMER: Q. All right. And so that we could look at it from the Ontario perspective, which is very reassuring, and then if one looks at it from the perspective of the U.K. studies where all of this originated, it is reassuring as well although perhaps not to the fullest extent that it is in Ontario; is that fair?

DR. WHILLANS: A. Well, as I said, I know there was a very thorough study of the discharges from Sellafield, which is the sort of major focus of concern in the U.K..

I am not sure how thorough this was done for Dounray - it was done, but I don't know the detail of it - and similarly for some of the other sites, and in the case of Sellafield it is certainly true that it is very unlikely that the discharges have anything to do with the cluster of leukaemias.

Q. You can't assist us on the other studies, the details of which you are not familiar with?

A. Well, for example, at Dounray there was a cluster, an increase in local concentration of childhood leukaemia, and papers have been published which looked at a number of factors, one of which was discharge estimates, but they were just one of the factors, and, you know, there isn't the detail to describe whether or not it was done as thoroughly as at Sellafield.

I think generally since the NRPB study of the Sellafield discharges there has been much less interest or concern about this as the source of the clusters. And other things have been looked at. The viral hypothesis is one.

One which I think we haven't mentioned is the question of paternal radiation, but since you raise the question of Dounray, that was one of the factors that was looked at. In that case it was found not to be significant.

So I think it is the case that most people would accept the discharges were not responsible for the major cluster, and although they are being checked in all these other circumstances it is not a major focus.

Q. By paternal exposure you mean the children whose fathers have had occupational exposure

in nuclear plants; is that correct?

A. Yes, that was the Gardner finding at Sellafield.

Q. Which was not borne out in the other study at Dounray?

A. Well, certainly at Dounray and at the others there was no significant association between the father's exposure and the probability of having a leukaemic child. But they are much smaller studies.

Q. Towards the bottom of page 1583 Mr. Barnes then turns to the bearing that this question of childhood leukaemia has on whether or not he should grant consent or recommend the granting of consent for that station, and says in the middle of paragraph 41.151:

> Since I consider it most improbable that an enhanced risk of leukaemia among children who live in the vicinity of nuclear plants is caused by discharges of radioactive materials from the plants or by any other general aspect of their operation I answer the question by concluding that there is no such likelihood.

And you would agree that that would be an

appropriate conclusion even more so to be drawn in Ontario, I take it?

A. I'm not sure what he means by: any other general aspect of their operation. But certainly I can agree with the rest of the statement.

Q. And he says towards the bottom of the page that:

> The question will continue to be asked until a complete scientific understanding of the disease is attained. It is perfectly proper that it should be asked.

And you pointed that out earlier when I was referring to the local councillor; correct?

A. Yes.

Q. And he says:

> The answer is a matter of judgment, not of logic. It is also an answer which in substance goes to the whole future of the civil use of nuclear power. My judgment is that the possibility is low and that there are substantial benefits to be obtained from continuing with the civil use of nuclear power.

I take it that that is the kind of judgment one makes in assessing all forms of energy

production and in particular the question of nuclear energy production; is that fair? One balances the benefits to be achieved--

A. That is right.

Q. --against the weight of the risks that are attendant?

A. Of course, yes.

Q. All right. I understand as well that in the United States similar studies relating to the risk of cancer in the vicinity of nuclear stations have been carried out and reported recently by an author named Jablon, of the National Cancer Institute? You are familiar with that --

A. Yes, I believe I referred to that in my direct evidence.

Q. Yes. And those studies related to 62 nuclear facilities and the counties surrounding them; correct?

A. That's right.

Q. And they came to a risk ratio of 1.0, which means normal; correct?

A. That's right.

Q. And, in fact, they are found that the risk ratio in those 62 counties was slightly higher statistically before startup of the nuclear facilities

than it was after?

A. I believe that's true.

Q. And you refer as well in your report to Atomic Energy Control Board studies here in Ontario, and I believe you may have referred to that in your evidence in chief when you testified orally.

A. Yes.

Q. And I'm sorry if I am repeating what has gone before, the results of those studies which was to find no differences achieving statistical significance in the areas around nuclear stations; is that correct?

A. That's right.

Q. Mr. Penn, are you able to tell us anything about operational safety of the nuclear plants?

MR. PENN: A. I think Mr. King can do that, or Mr. Daly.

Q. Well, Mr. King and Mr. Daly, could we turn to tab 14, which again is the excerpts I have put together from the Hare Report, and I am referring to tab 14 in Volume 2, and Roman numerals page 12.

Mr. King, under the heading Risk of Accidents, Commissioner Hare found a severe accident in an Ontario reactor with release of damaging amounts of

radioactive substances is very unlikely but cannot be ruled out.

And you may have read from that in your evidence in chief, but I take it that you agreed with that conclusion when it was released and you would accept that as a valid conclusion today?

MR. KING: A. Yes.

Q. And two paragraphs down: The more serious incidents have -- and I am not reading here, I am paraphrasing.

The more serious incidents in Ontario's nuclear stations have been those at Pickering "A" in 1983 and Bruce "A" in 1986?

A. I referred to the Pickering "A" incident in my evidence in chief.

Q. All right.

A. And I am aware of the event that he is talking about in Bruce "A" in 1986.

Q. Am I correct that the one difference between those is that the Pickering "A" incident happened when the plant was in operation, in normal operation producing power, whereas the Bruce "A" incident the plant was either shut down or at very low power levels?

A. The plant was shut down.

Q. All right. And in neither case were the safety shutdown systems called into use to prevent the release of radioactivity?

A. If you are referring just to the shutdown systems...

Q. The special safety, the independent shutdown systems.

A. In Pickering "A" the reactor was shut down manually using the regulating system, hence the shutdown systems--

Q. The normal --

A. --were not used.

Q. Right.

A. And in the Bruce "A" incident, as we have noted, the reactor was already shut down.

Q. And there has been no subsequent incident in which the special shutdown systems were called upon to prevent the release of radioactivity into the environment at any of Hydro's stations; is that correct?

A. To my knowledge, I believe that is true. Occasionally there are spurious trips of shutdown systems which shut down the reactor by the mechanism, but they weren't there and they didn't occur in response to an accident event.

Q. And by accident we can mean a system failure sufficient to trip the shutdown system?

A. That's right.

Q. If we can turn over in the further excerpts from Dr. Hare's report at tab 15, which is excerpts from the second volume, I would like to refer you to page Roman 1/63, which is an excerpt from Mr. Meneley's paper which was published as an appendix to the Commissioner's report.

Do you have that?

A. Yes, I do.

Q. And Mr. Meneley was with Ontario Hydro and then went to the University of New Brunswick, as I understand it, and is now with AECL?

A. Yes. Yes, he is.

Q. And at the time he wrote this paper was he with UNB?

A. Yes, he was.

Q. And he says at paragraph 77, and we have been over some of this in your evidence in chief, that the philosophy guiding the plant designer is one of defence and depth, and the defences range from preventive measures, such as high quality hardware and well-trained, highly motivated staff to mitigative measures such as those represented by special safety

systems and establishment of an exclusion zone around each nuclear plant.

And you went through that kind of description in your evidence in chief?

A. I referred to all of these features. When I was talking about the defence and depth I was going through an explanation of the physical barriers, but defence and depth is also discussed in these terms as well. It can be discussed in these terms as well.

Q. It is not only a question of the physical design of the plant but the institutional culture, if you like, that prevails within the plant or the human side of things?

A. That's true.

Q. And the idea on both sides is that one recognizes from the outset that in any technological process systems can fail?

A. That's right. You have overlapping redundant ways of achieving objectives or preventing things from happening, and that is a general description of a defence and depth.

Q. So that if the failure occurs one has already made provision in the design to mitigate the effects of that failure?

A. Yes.

Q. That applies not only to nuclear power stations but to all sorts of industrial activities; correct?

A. That's true, and I believe I mentioned that in my evidence in chief as well.

Q. And at paragraph 78 Mr. Meneley makes the observation:

> The underlying reality in the design of any safety system, whether it be in homes, automobiles, aircraft or nuclear power plants, is that the most severe possible accident can occur at some probability. The objective of the design is to make this probability acceptably low. It can never be zero.

And you would agree with that?

A. I would.

Q. And then the difficult part comes next: The value judgment word is "acceptably". And you would agree with that?

A. Yes.

Q. A real or perceived risk is accepted by society in return for some real or perceived benefit.

And that is obvious, isn't it?

A. Yes.

Q. And dropping to the bottom of the page:

> Objective measures of safety can be compared easily with alternative ways of achieving the same benefit, but perceived risks and benefits vary considerably with time and with the political mood of the society. Today's 'acceptable' level of safety could be tomorrow's 'not enough', and the next day's 'too much'.

And you have observed in the course of your career fluctuations of that kind, I take it, in relation to nuclear safety?

A. Well, I certainly see the first part of the equation where today's is tomorrow's 'not enough'. That seems to be the dominant trend.

Q. But you would agree with Mr. Meneley that the question of too much, enough, or not enough is as much a matter of perception as objective measurement?

A. I would think the lack of objective measures or the lack of accepted comparisons leads to the reliance on perceived values rather than on objective values which...

Q. Right. And Mr. Meneley concludes that paragraph:

> High levels of safety can be achieved but at significant cost and the society must decide the acceptable balance between cost and benefit for regulated activities such as nuclear energy.

And, in fact, that is part of what we are engaged in here in this hearing, isn't it?

A. That's true.

Q. And then in the next paragraph he states:

> The most fundamental reality of nuclear power plant operation is that the maximum consequences of any severe accident are limited. They can never be large compared with other potential events such as hydraulic dam failures or poisonous chemical releases.

You would agree with that, would you not?

A. I guess I have a little bit of difficulty with a few of these words in here: The maximum consequences of any severe accident are limited? I guess it really depends what 'limit' is. If you want to establish your limit high enough, then I

assume that the consequences of anything are bounded by that limit.

Q. Well, he makes the obvious point in the next sentence:

> A nuclear power reactor absolutely cannot explode like a nuclear weapon.

[3:05 p.m.]

A. I agree with that.

Q. And nor can it release enough energy or fission products to produce devastation equivalent to such an event?

A. Also when he is talking about consequences, maybe he is clear in earlier paragraphs, but in this one whether it's health effect, prompt fatality, latent fatality, contamination, I am not sure exactly what he is referring to, and hence when you start comparing to other forms of energy production, you have to make sure you are comparing apples with apples.

Q. Right. But that applies to any form of energy production that one can think in terms of say an immediate fatality in a coal mine accident or one can think of delayed fatalities due to lung disease caused by inhalation of coal dust. That's fair; isn't it, the same distinction that you just drew?

A. I think both are consequences and in my belief both have to be treated separately. There is a different impact from both latent and prompt health effects.

Q. And one has to assure that the likelihood of such effects is acceptably low.

A. That's true.

Q. But one has to recognize that any industrial activity does carry with it a percentage risk of such effects or similar effects?

A. That's true.

Q. May we turn, Mr. King, to tab 10 in my Volume 2. Have you had an opportunity to review this booklet entitled Safety of CANDU Nuclear Power Stations by Dr. Snell?

A. I have read it in the past when it was first written and have scanned it in the last couple of days, yes.

Q. And I am going to take you to some specific passages in it, but in general it is a useful description of the safety systems which are designed into CANDU stations; is that fair?

A. Yes.

THE CHAIRMAN: Give it a number.

THE REGISTRAR: 557:

MR. HAMER: Thank you.

---EXHIBIT NO. 557: Document entitled Safety of CANDU Nuclear Power Stations, by Dr. V.G. Snell.

MR. HAMER: Q. At page 7 of this booklet we see a section entitled Measure of Risk, and that's part of your job in relation to Ontario Hydro's nuclear stations; is that fair?

MR. KING: A. Yes.

Q. Dr. Snell writes:

> This section describes the amount of risk implied by the operation of a nuclear power plant. Before we protect against particular accidents we must decide how safe we want the plant to be. Nothing can be made absolutely safe and the safer we try to make the plant, the more it costs in terms of safety devices and reduced output. Indeed, beyond some point the improvements in "real" safety may be illusory.

Would you agree with that observation by Dr. Snell?

A. Certainly as you add more devices there are cost implications. It does not necessarily imply reduced output.

Q. But it may?

A. It may, but it's not a given.

Q. Right.

A. There are some points where if you improve the safety, you are at very low levels of risk, the uncertainty in the gain that you are actually making or the gain that you estimate that you are making may well be swallowed up in the uncertainty around that mean value you may be using.

Q. For example, if one were to spend a million dollars to reduce the risk of an accident from one in one million years to one in 10 million years, one may be wasting that million dollars; is that fair?

A. It would probably be hard to justify those sorts of expenditures for those levels of risk reduction.

Q. And then Dr. Snell writes in the next paragraph:

> Without numbers for risk, it is as hard to compare the safety of competing energy technologies as it would be to compare their economics without knowing their cost.

Would you agree with that logic?

A. Yes, I would.

Q. Sometimes such quantitative studies give surprises. It is hard to see much risk in solar power and perhaps easier in nuclear power. Yet Inhaber who studied both in an exploratory analysis, claimed the reverse: he have claimed that the public risk of death and of man days lost is higher for solar power than nuclear. The main reason is the enormous amount of materials needed for the "benign" technology - these must be manufactured and transported and that is where most of the risk lies.

Are you familiar with those kinds of studies and Inhaber in particular?

A. I am aware of the Inhaber study that he is referring to. I know nothing about the estimates of risk from solar power. I make no comment on that.

Well, perhaps the only comment that I would make is that if you are looking at the alternatives, you should look at all aspects of all alternatives.

Q. Right. And that passage makes that point that one doesn't simply look at the presence of solar panels on the roofs of buildings but what it

takes to get them there and the activities associated with that; correct?

A. I think that's the same as the statement I just made.

Q. You got it.

Inhaber is a well recognized authority in the area of risk analysis?

A. At the time when he prepared -- I am not sure which study this is referring to.

Q. 1978.

A. He was a bit of a pioneer, I believe, or one of the first people doing this sort of calculation. I am not sure if he is still publishing in the area or not, but certainly at that time. In fact, he was a member of the Atomic Energy Control Board at that time, I believe.

Q. Again, we find another one of his publications in Ontario Hydro's list of references in Exhibit 507, and that's entitled Energy Risk Assessment.

Are you familiar with that publication?

A. What is the year of that?

Q. 1982.

A. I just know that he has published in that period, late 70s, early 80s, in this area. I

can't confirm that I actually read that particular one.

Q. And then Dr. Snell goes on at the top of page 8, left-hand column:

> In short: we cannot make a human activity absolutely safe. We can improve its safety. This requires social resources. At some point society must judge when the level of safety is good enough so it can use these resources elsewhere.

And that's an important point to recognize; is it not?

A. There is, I guess, an implication in this sentence that the society has the vehicles available for transferring resources from different parts in society to make that balance. It should be spent here rather than here. I am not sure whether those societal vehicles for doing that are in place and effective.

Q. We have to make use of the imperfect institutions that we have. For example, many of the issues in this hearing involve making those kinds of choices with respect to allocation of resources; do they not?

A. I would think in this area one should

establish reasonable levels of acceptable safety, and if those reasonable levels were applied across the broad range of societal activities, then there would be a reasonable balance across society for the spending of those resources.

Q. In fact, that is a problem, is it not, in that some industrial activities through perhaps misperception of the risks attached to them, end up having more resources devoted to making them safer than are actually justified, while other industrial activities go under-regulated, if you like, from the point of view of safety.

A. There is certainly a lot of material in the literature which suggests that that is the case.

Q. Dr. Snell goes on in the next paragraph to give the simple example of whether or not seatbelts should be used in motor vehicles and says that sometime ago it was fine not to drive with seatbelts and now its compulsory. And that's a way of reducing the effects of an accident if the accident happens and it requires an expenditure of money, not very much, installation of a seatbelt and the enforcement of the Highway Traffic Act.

A. Yes.

Q. He goes on to make the next obvious

analogy that one could spend much more on better road design and improved driver training and avoid a great many further fatalities.

A. I would assume that's probably the case, but I have no detailed knowledge of the size of the benefits that could be obtained for any expenditure.

Q. That's fair.

But you will agree with me that that's an apt example of the perhaps disproportionate allocation of resources to extend life expectancy?

A. Well, if society wanted to reduce the number of -- if we are looking at the consequence of prompt fatality, of course one of the things you would do is take a look at the past data, figure out where the largest number of people are being killed from an actual point of view, and try to apply your resources in those areas, because you would probably get the best bang for your buck, as the expression goes, by doing that.

Q. I know you haven't done a review of the data, but would it be reasonable to think that in our industrial economy far more fatalities have already been caused in history by motor vehicle accidents than will ever, ever be caused by nuclear power plants?

A. Well, ever ever is a long time.

In my lifetime, yes.

Q. Certainly in the next 25 years.

A. I hope I make it that long.

Q. And over at page 10, Dr. Snell sets out a table of individual risk of early fatality by various causes, and I am sure that you have seen this kind of comparison before. It's at page 10.

A. Yes, I have got it.

Yes, I have seen it.

Q. We see motor vehicle accidents, and these are U.S. figures but one wouldn't expect Canadian figures to be substantially different; would one?

A. There are a few of them which -- the order may not be exactly the same.

Q. Firearms might be a little different.

A. Yes, firearms, drowning. But generally they would be the same.

Q. So that we see that motor vehicle accidents, one has in the United States, at least, a risk of 1 in 3,000 per year of early fatality.

A. I see that.

Q. And that's not a surprising figure to you?

A. No.

Q. And air travel carries a risk of 1 in 100,000 per annum, and that's not surprising to you?

A. The air travel number varies from year to year much greater than, let's say, the motor vehicles would vary from year to year.

I assume this is one year, for 1969?

Q. The column on the right-hand side says chance per year of early fatality.

A. What I am saying, I am not sure over what year period this is the average. But yes, in the air travel it's in the right order, I would guess.

Q. All right. And one has, according to this table, a risk of one in two million of being struck by lightening and being killed. Is that a reasonable figure to you?

A. I'm sorry, I just don't know how many people are killed by lightening. It says 160, but I take it that that's an accurate figure.

Q. At the bottom of the table we have nuclear accidents spread over 100 reactors apparently and the risk given there is 1 in 5 billion; is that correct?

You deal with these numbers more than I do.

A. I was just trying to check what

reference they are using.

They are using the 1975 WASH-1400 Rasmussen Reactor Safety Study, is the reference.

Q. And that's one study among many and they come up with different answers. But in terms of order of magnitude that figure is a reasonable one?

A. They are talking about an individual, this is the average individual, I believe.

Q. Over a year?

A. Once you are talking about fixed facilities, then really there is a large variation in the risk from the person on the boundary to the person who is 200 miles away from the nearest nuclear reactor. They are both small but there is still a large difference between them in both.

Q. And apparently according to the footnote, the author of this table has estimate that figure based on a population of 15 million at risk from a nuclear accident, and presumably has spread it over these hundred reactors.

A. It appears that they have taken a certain distance from a reactor and that would have included 15 million people and then taken the average within that distance.

Q. But in terms of putting the risks of

nuclear power generation into one perspective, this table is a reasonable way to go about doing it; isn't it?

A. What you are comparing here, though, in some cases you are comparing past statistics which have very low uncertainty bounds on them, compared to estimated probabilities, estimated risks, and you have to be a bit careful when you do that to make sure, as I mentioned before, that the uncertainty bounds around your estimates don't effect the comparison that you are trying to make.

Like in this particular case, the events at the top of the list are much more frequent, and it's probably a fairly safe comparison when you are talking about events at the top of the list.

Q. I am interested obviously in the event at the bottom of the list, the nuclear accident.

A. What I meant was the other list, the list where they had statistics, hard statistics. The list above the middle line.

Q. I'm sorry?

A. I believe that's the distinction. Between the first two horizontal lines are actual statistics and the nuclear accident is an estimated risk.

Q. Based on probability analysis?

A. Based on the WASH-1400 Reactor Safety Study, probabilistic risk analysis.

Q. Yes. And you are in the business of making those probabilistic risk assessments, aren't you, yourself?

A. Yes.

Q. And what I am trying to get at is that accepting the distinction you make between historical data and probabilistic risk assessments and accepting the frailty of comparing one to another, that is nonetheless a reasonable way of putting the risk of nuclear accident into some kind of valid perspective?

A. It is, with the cautions I have mentioned on uncertainty.

So if you are comparing it to the lightening or the tornadoes or the hurricanes, I think you would be a lot more in making the comparison then the motor vehicles, falls, et cetera.

Q. I think I understand you now.

I want to turn very briefly to the Atomic Energy Control Board as a regulator, and that discussion I will begin with a reference to the excerpts from the Hare commission report at tab 14 in my Volume 2.

[3:29 p.m.]

I am going to start on Roman numeral 18.

Actually, before we go to Roman numeral 18, I will start at page 190, and then we will come back to Roman numeral 18.

Mr. King, are you the appropriate individual to answer questions relating to the AECB?

A. Yes.

Q. And Dr. Hare asks the question on page 190: Is AECB sufficiently visible? And the answer to that as of that time:

> The answer is clearly no. The average citizen is entirely unaware of the watchdog function performed on his or her behalf.

This is at the bottom of that page.

> In fact, outside the nuclear community itself AECB is almost unknown amongst influential groups in Canada and usually confused with AECL when the subject is raised.

Would you have an opinion on whether that profile has changed since the Hare Commission released its report?

A. Well, I would like to make the comment beforehand that the AECB is certainly very

visible to us.

The AECB over the last few years have been moving to open up their activities. I believe they have a lot more of their meetings, the board meetings are open to the public, and they are moving the board meetings away from Ottawa. I believe they just had one fairly recently in I think it was Saskatchewan. I'm not sure.

But they are moving it out into some of the local communities. So I would agree with the fact -- your statement, I believe it was, that they are becoming more visible.

Q. And Dr. Hare refers in the next paragraph to two key safety-related advisory committees of the AECB, and he describes them as highly expert bodies which offer the AECB excellent advice and prepare definitive statements for publication.

And he cites two of those publications, one being ACNS-10, Alternative Electrical Energy Systems: A Comparison of the Risks of Occupational and Public Fatalities, and I believe that document is referenced in your Exhibit 507, is it not?

A. Yes, it is.

Q. And he says that:

The work of these committees is

admirable and the reports are as good as anything I have seen, but they remain little read, are circulated primarily within the nuclear community, and are slow to appear even within this restricted circle.

I am not certain at this point as to how broadly known those reports are, but would you agree that the work of those committees, and in particular those two publications there, are admirable?

A. The two publications, you are referring to the ACNS-4, which you didn't mention, and ACNS-10?

Q. Actually, let's just refer to ACNS-10.

A. I have certainly read this report. It appears to be a good report to me, but again, as I mentioned earlier, I am not an expert in the non-nuclear aspects of the material that they covered in that report, so I would have some difficulty judging the goodness of that work.

Q. And Dr. Hare says in the next paragraph:

> Neither committee seems to feel that it has a responsibility to reply to the

frequent allegations by anti-nuclear groups that risks are being underestimated and disquieting evidence ignored. In many ways I support the Committee attitude, which is a normal scientific position, but it leaves a vacuum on the public scene which extreme opinions rush in to fill. The result is doubt, anxiety, and bewilderment in the public's mind. Politicians confronted with this vacuum have no clear way of getting a dispassionate judgment.

And that is another way of referring to the difficulty we were discussing earlier of an absence of objective bases on which to make energy choices as compared to subjective perceptions, isn't it?

A. Well, I would agree with the statement that is in this report, that these committees probably do not have a very high profile in the non-nuclear community.

Q. And you would agree that in some ways there is a vacuum left on the public scene which extreme opinions rush in to fill?

A. Well, I am sure we are all aware of that happening, yes.

Q. And then going back to Roman numeral 18, Dr. Hare concluded at, I think it's Commission recommendation 11.4, if I am reading the shortform correctly, that:

> AECB is an effective regulating agency. It sets the conditions that guarantee safety to the public and the work-force and leaves it to Ontario Hydro to show that its designs and operating methods are able to meet these conditions.

I take it you would agree that that conclusion was valid when it was issued and remains valid today, that the AECB is an effective regulating agency?

A. I believe it is.

MR. HAMER: Mr. Chairman, I am going to turn to a new topic. I don't know -- frankly, I have forgotten when you usually take a break.

THE CHAIRMAN: Well, it is around now so we will take it now, 15 minutes.

THE REGISTRAR: Please come to order. This hearing will take a 15-minute recess.

---Recess at 3:35 p.m.

---On resuming at 3:53 p.m.

THE REGISTRAR: This hearing is again in session. Please be seated.

MR. HAMER: I am in tab 14 of Volume 2 dealing with the Chernobyl accident.

THE CHAIRMAN: What page?

MR. HAMER: Page 157 and -8, and 157 will probably be a loose insert which I didn't put in the first time around, and I have merely inserted that just to provide some context.

I am actually going to start on page 158, which is bound in, but 157 is provided just to put that into context.

Q. Mr. King, I think you may be the appropriate person to start with. Dr. Hare concludes at page 158 about five lines down...

I think you will find 158 is bound into the book, sir. Oh, I see.

THE CHAIRMAN: No, it is not. Not in my book anyway. I've got it. It is not bound in, that's all.

MR. HAMER: Okay? I think we have got several varieties, Mr. Chairman.

MR. KING: I have got page 158.

MR. HAMER: Q. Okay. And Dr. Hare concludes in this passage of his consideration of

Chernobyl:

> Clearly, the Pickering "A" event is a pygmy by comparison with that at Chernobyl primarily because of the much lower mass of coolant available at Pickering for the destructive blowdown and because of the very quick termination of the power transient due to loss of moderator. That plus the use of an inflammable moderator at Chernobyl accounts for the better survival expectation at Pickering.

There is a lot contained in those sentences, but, first of all, the Pickering "A" event is a probabilistic analysis similar to what we were discussing before the break, and I think we discussed it perhaps yesterday as well; correct?

MR. KING: A. No, I believe they are referring to the loss of coolant, loss of shutdown analysis that was performed by Ontario Hydro and Argonne.

Q. That is what I meant. It is a probabilistic study?

A. We wouldn't call it a 'probabilistic' study. The event was defined, irrespective of the

probability of it occurring, and say, could you please analyse that -- Dr. Hare said, could you please analyse that event, and what would be the consequences of that event?

Q. And the event --

A. Irrespective of probability.

Q. Okay. That is helpful. And the event was what?

A. It was a large loss of coolant accident, which is a rupture of the largest pipe in the heat transport system of Pickering "A", combined with the failure of the shutdown system in Pickering to operate.

Q. And we discussed yesterday I think the fact that it was directed to Pickering "A" because it had only one shutdown system; correct?

A. That's correct.

Q. And Dr. Hare terms the event predicted and described in that study as a pygmy by comparison with that of Chernobyl?

A. That is what it says.

Q. I bet you like that fine. And that is premised, first of all, on the fact that there would be a much lower mass of coolant available at Pickering for the destructive blowdown. Do those terms mean

something to you, and if so, what do they mean?

A. Yes. Well, in fact, I believe in this material in the handout there is a graph, some bar charts at the end which illustrate these points where the mass of coolant -- yes, that is it.

Q. Page 159.

A. The significance of that is with respect to the integrity of containment, the mass of coolant available in the heat transport system which would blow down and the temperature and pressure of the enthalpy of the cooldown, of the liquid steam mixture that is being blown down, that is discharged into the containment atmosphere. That combined with the volume of the containment atmosphere would lead to a rise in pressure of a certain amount.

If you look at the factors which affect the rise in pressure in containment, the mass of coolant available, the volume of containment, the Pickering situation is a lot better than the Chernobyl situation.

Q. Why do you use the word blowdown? Blow where?

A. It is just the word that is used. If you have a pipe full of high pressure, high temperature water, and if there is a rupture in that pipe you just

picture, you know, a line with hot steam in it. It just comes out as mixture of steam and water, and that is just referred to as 'blowdown'.

Q. And at Chernobyl was there a steam explosion as a result of the blowdown?

A. No.

Q. There was no steam explosion?

A. Steam explosion in nuclear safety analysis refers to the process of where a very hot solid compound meets a liquid compound and through the fast generation of steam causes a steam explosion. That --

Q. Sorry?

A. That phenomena, to my knowledge, was not an important phenomena at Chernobyl.

Q. We might come back to that a little later. In any event, the volume available to blow down is far smaller in the Ontario Hydro station as we can see on the left-hand side of the bar chart?

A. Yes.

Q. And Dr. Hare then refers to the very quick termination of the power transient due to loss of moderator. And what does that mean to you?

A. In the analysis that we performed, what happens early on in that transient when you have

failure to shut down is that the heat transport coolant becomes very hot, very high pressure. The energy in the fuel becomes very high. This leads to a rupture of the channels, of some channels.

If you recall our description of the core of a CANDU reactor and the pressure tubes and calandria tubes going through it, there is ruptures of the pressure tubes and calandria tubes and the blowdown of the coolant coming from the heat transport system into the moderator -- at Pickering there is a moderator dump system, and, in fact, it will eject the moderator out of the calandria vessel and shut down the reactor at a very early time in the transient.

Q. And sometimes in the literature you see that dump of the moderator referred to as a shutdown system in itself, although it is not independent of the generating process?

A. That's right. And the difference with Chernobyl is their moderator is graphite. And graphite, it is in the form of large, very solid heavy blocks of carbon, graphite is, and there isn't the same capability to dislodge the moderator at Chernobyl.

Q. And, in fact, those blocks of graphite are very hot in normal operation and in an accident situation such as occurred if there is an

explosion that permits air to come into contact with the graphite it burns?

A. That's correct.

Q. And many of the 31 who died were very seriously burned as a result of the fires started by the graphite?

A. That is my understanding. I don't know which percentage of the 31, but I understand it was a significant factor in their deaths.

Q. Presumably they also had very high radiation exposure as well?

A. Yes.

Q. Is that fair?

A. Yes.

Q. Could we turn then to Roman numeral 13, and CR8.5 sets out the formal conclusion of Dr. Hare:

> If a severe accident were to occur it would be quite unlike that at Chernobyl in 1986. The Chernobyl reactor had seven times as large a coolant volume available for blowdown and used inflammable graphite as a moderator.

And then he refers to the point that we just made concerning the burns from graphite fires.

And then he says:

> Other severe accidents can, however, be visualized in CANDU reactors. Two have been identified by AECB. These were failure to shut down following a large loss of coolant or a loss of regulation.

And the former of those two visualized severe accidents is what was studied in connection with the Hare Commission; is that correct?

A. That is true.

Q. Was the latter studied subsequently?

A. We have not performed an analysis to the same extent as for the large loss of coolant, loss of shutdown.

Q. Have you done any work in that area?

A. Well, there is loss of regulation studies done in all our safety reports for all our stations, and actually, it was probably around the time of the Hare Commission where all of that analysis was updated for all the stations, all the loss of regulation analysis, which would search out opportunities and situations where loss of regulation accidents could occur, and they would all be documented in the individual safety reports for all the stations.

[4:05 p.m.]

Q. You are satisfied with the status of all stations in that respect?

A. Yes, as I said, these are all done and submitted to the AECB.

And in that analysis all the relevant criteria or acceptance criteria in the regulatory environment were met.

Q. As part the continuing licensing process?

A. Yes.

Q. I want to refer to Exhibit 507, and I hoped that another loose page has been inserted, page 519 in my excerpt at tab --

A. Just to clarify, I didn't want to leave, I think I may have left the wrong impression.

It's the loss of regulation analysis that is performed. It does not, in all the safety reports, include a failure of shut down.

Q. No, I appreciate that.

At tab 8 of Volume 2 we have the excerpts from Exhibit 507 and you may want to slip in the loose page 519 which I am going to refer to briefly.

Do you have that, Mr. King?

A. Yes, I do.

Q. In the middle paragraph of that page

we find a part of the discussion about the various fatality risks relating to the various stages in the nuclear fuel cycle, and the statement appears:

> Attempts have been made to use the consequences and the single occurrence of the Chernobyl accident to estimate the risk from severe accidents.

You refer there to the Helsinki symposium of 1991. We don't need to turn to the footnote, but I think it is an excerpt from something called Key Issues Paper No. 3.

Are you familiar with that document?

A. Well, you handed us a key issues paper, I am not sure whether it's No. 3 or not.

Is this the one that you are referring to?

Q. Yes. I don't know if we have provided copies of that to the Board as yet.

THE CHAIRMAN: If you have it, it escaped my attention.

MR. HAMER: Sorry, we have to locate that to follow this, Mr. Chairman.

THE CHAIRMAN: This should be an exhibit, I take it.

MR. HAMER: It's referred to in Ontario

Hydro's list of references in Exhibit 507, so presumably it could be entered as one of those references.

THE CHAIRMAN: They are not all exhibits, I don't think.

MR. HAMER: No. I would like to have it entered, Mr. Chairman.

THE REGISTRAR: Is this to be made an Exhibit, Mr. Chairman?

THE CHAIRMAN: Yes.

THE REGISTRAR: 558.

---EXHIBIT NO. 558: Senior Expert Symposium on Electricity and the Environment, Helsinki, Finland, 13-17 of May, 1991. Key Issues Papers.

MR. HAMER: Q. This is an excerpt from Key Issue Paper No. 3, and I have the whole of the volume here, if anyone wishes to refer to it. It covers a great many other things.

Is this the table from Key Issues Paper No. 3 which is referred to in Exhibit 507, Mr. King?

MR. KING: A. I would have to check that, unless some of my colleagues know.

Q. You will see at the bottom of the page there is a reference to nuclear as a source of an energy-related accident and then under the column

location there is an entry --

A. Which page are we talking about?

Q. I am looking at the table on page 131.

A. Okay.

Q. At the bottom of that table you see nuclear, location Chernobyl, country USSR, et cetera?

A. Yes.

Q. And then there is figure for immediate fatalities, occupational 31. And then they have not entered a figure for late effects. And then over the page, two pages on we find table 17 headed Normalized Fatality Rates for Severe Accidents, 1969 to 1986, and there again at the bottom of that table we see an entry for nuclear?

A. I see that.

Q. And as I understand this table, it's taking historical data relating to severe accidents in different kinds of energy production facilities and it attempts to come up with a fatality rate for such severe accidents for the various energy processes.

Do you see that?

A. They are coming up with a figure in units of immediate fatalities per gigawatt-year.

Q. And that is the same kind of ratio

that we find throughout Exhibit 507 with respect to the various phases of the nuclear fuel cycle; is that correct?

A. Yes.

Q. As the table points out in the footnote, we are dealing only with immediate fatalities and not delayed fatalities for all of these figures; correct?

A. Yes.

Q. And we see that on the basis of the Chernobyl accident, which is the only one that has caused fatalities during the time period 1969 to 1986, the figure of 0.03 fatalities per gigawatt-year appears.

A. I see that.

Q. And would you accept that as being an accurate figure given its source?

A. I have looked at this table before and it is energy produced column - I assume they have done their math correctly, but I am not sure what this energy produced is. Is this all nuclear power?

Q. It's my understanding that it is 1,100 gigawatt-years which attributable to the Chernobyl station. I maybe completely wrong about that.

A. No.

Q. That would be 1,000 megawatts?

A. The Chernobyl reactor would be - if it ran at full power a full year - would be 1 gigawatt-year.

Q. I am told that it is probably all nuclear generation and the fatalities at Chernobyl are in effect spread over all nuclear energy generated for a year. Would that make sense to you at 1,100 gigawatts?

A. Well, that would assume that there is 1,100 thousand megawatt reactors around the world, which seems a bit high to me.

Q. I said for a year, it's really 1969 to 1986; isn't it?

A. But the units are per annum.

THE CHAIRMAN: The units are GWa.

MR. B. CAMPBELL: I am not sure it is.

MR. KING: I see.

MR. B. CAMPBELL: In any event, Mr. Chairman, I am not sure how Mr. King can be asked to verify a figure of the total amount of nuclear electricity produced over all operating reactors over their whole operating history off the top of his head. I am sure that this at least is not a number he carries

around with him.

THE CHAIRMAN: In the whole world.

MR. B. CAMPBELL: In the whole world, that's right.

MR. HAMER: Nor do I ask that question, Mr. Chairman.

MR. B. CAMPBELL: He already said the math is right, he assumes. I don't think he can help my friend in confirming that figure.

MR. HAMER: Q. In Exhibit 507 the statement appears:

> Attempts have been made to use the consequences in the single occurrence of Chernobyl accident to estimate the risk from severe accidents.

And this is the document.

MR. KING: A. But these are not our attempts.

Q. I appreciate that.

I am just trying to understand the document to which Ontario Hydro refers in its exhibit.

Mr. Johansen, can you help us on that?

A. If you go back to the reference, if you go to the reference in Exhibit 507, it refers to Key Issues Paper 3.

Q. Right. And that's what we are looking at?

A. Yes.

Q. And do you have any understanding of the document to which this exhibit refers, and in particular the table which appears to attempt to use the consequences of Chernobyl to estimate the risk from severe accidents?

A. If you continue in that paragraph in 507, the one starting with the word "Attempts" into the next couple of sentences, I certainly agree with the argument being presented here, in that you can't take the Chernobyl reactor to be representative of all other reactors in the world in particular the ones that are here.

Q. We are going to come to that. We are going to spend time on the fact that Chernobyl is not representative.

A. And given that, then it is from a Canadian perspective, that is not a good way of looking at the statistics.

Q. But all we know in terms of historical data on fatalities resulting from nuclear accidents in power generators comes from the Chernobyl accident; does it not?

No one was killed at Three Mile Island?

A. If you are looking at immediate fatalities then that's true.

Q. Right. And what the authors of this table appear to be doing is taking the number or the amount of energy produced by nuclear power in the world and simply taking the deaths at Chernobyl and attempting to come up with a fatality rate per gigawatt-year?

A. They have taken a world perspective.

Q. Right. And come up with a figure 0.03 fatalities per gigawatt-year for nuclear?

A. Yes.

Q. And again, those are prompt fatalities?

A. Yes.

Q. As compared to 1.41 fatalities per gigawatt year for hydraulic power?

A. That's what the table says.

Q. And 0.17 fatalities per gigawatt-year for natural gas?

A. Again that's what the table says.

Q. Would you have any reason to thing that the Helsinki Symposium authors were not relying on appropriate literature reviews and other studies?

A. I have no reason to believe that.

Q. All right. And coal is calculated at 0.34 fatalities per gigawatt-year?

A. Yes.

Q. But as you pointed out a moment ago, you wouldn't accept that figure of 0.03 fatalities per gigawatt-year for nuclear in Ontario, because Ontario reactors are not like the Chernobyl reactor; fair?

A. Yes, and I wouldn't accept the other figures for the same reason as well.

Q. Are you saying you know something about the characteristics of hydraulic installations --

A. No, but this is a matter of principle.

Q. What principle is that?

A. That if you are trying to predict what are the risks in the future, then you want to take the data which is representative of those facilities that you will have in the future in order to make that prediction. And that's the point I was making with the nuclear power and hence you would have that same principle with respect to anything.

Q. But one of the criticisms, Mr. King, of predicting the future and future fatality rates is that there is no historical evidence on which it's

based, if you are about talking a theoretical future power generating facility. Is it not appropriate as one way of looking at the problem to look at what has happened in the past with various kind of energy production?

A. I go back to my point, it depends what types of facilities those data were collected on in the past and what typed of facilities you are using that data for to predict the rates in the future.

Q. And as a rough rule of thumb, however, one can get some useful information by reviewing the literature as Ontario Hydro has done in it's Exhibit 507.

A. The greater the difference between what is in the past and what is in the future, the rougher that figure will be.

Q. But that's the only historical data you have; isn't it?

A. My point with respect to the nuclear is that I would expect the rates to be lower than --

Q. We don't have any disagreement on that.

But with respect to the other forms of energy production, it's appropriate to look at fatality rates recorded in the past.

THE CHAIRMAN: You can't have it both ways, Mr. Hamer. You can't say we must look at it because we have better technology mere, but then look at world technologies such as hydroelectric in India, for example, and say that that's got to be compared. That's an apples and oranges situation. I would think, it seems to me to be that.

MR. HAMER: Q. Is that your point, Mr. King.

MR. KING: A. That's exactly my point.

Q. So you are saying there is no point in looking beyond Ontario or Canada for comparing energy alternatives --

THE CHAIRMAN: No, he didn't say. In fairness to him he is not saying that.

He is saying if you are going to look at risks you should look at comparable technologies and see what the risks historically have been there, not just a blanket over the world of hydroelectric or coal, when you don't know what the technologies are.

I think that's what he was saying. At least that's what I understood him to be saying.

Is that the correct, Mr. King?

MR. KING: Yes, I think that is a good summary of what I was saying.

MR. HAMER: Q. Then as far as we can take these kind of world figures is to say that of the technologies surveyed world-wide in this table, in the Helsinki paper, nuclear comes out the lowest in terms of fatalities per gigawatt-year in comparison with the other energy alternatives for prompt fatalities?

MR. KING: A. That's what the table says.

THE CHAIRMAN: Actually, it's the second lowest.

MR. KING: I believe it's the lowest, Mr. Chairman.

MR. HAMER: I think, Mr. Chairman, you may be looking at 0.02.

THE CHAIRMAN: Isn't that lower than 0.03.

MR. KING: That is a sub set of the oil.

MR. HAMER: That's only refinery fires. You have to add capsizing and transportation.

Q. Is that correct, Mr. King?

MR. KING: A. That's my understanding of the table.

DR. WHILLANS: A. Can I make an unsolicited comment?

I think it is unreasonable... .

MR. B. CAMPBELL: Once again, they never pay any attention to what their lawyers tell them. [Laughter]

MR. HAMER: Because it's almost at the end of the day, Dr. Whillans, I won't object.

DR. WHILLANS: This table refers to, as you kept pointed out, prompt fatalities, but I think it is unreasonable - and it says say in the text - to ignore the fact that there may well be additional facilities as a result of Chernobyl.

MR. HAMER: Q. Absolutely. And there may be delayed fatalities from other accidents in other energy --

DR. WHILLANS: A. That's true. But without knowing much about it, I would say it would be less likely for hydro power. Those are acute events.

Q. How about coal?

A. I don't know about coal. There may be a mix.

Q. And you don't really know about delayed fatalities or shortened life expectancy from injuries suffered in hydraulic projects, for example?

A. That's true. That's true.

Q. But these are fair comments, these numbers are never precise measurements, they are simply

one indication that can be referred to in making the kinds of choices that have to be made in this case.

A. Yes, I agree. I just wanted to emphasize what you did say, which is that these are only the acute fatalities.

Q. Right. And another thing that one can draw from these figures, acknowledging that they are not precise measurements, is that nuclear power is not some extremely dangerous form of generating electricity in comparison to other alternatives; fair?

MR. KING: A. Are you asking me?

Q. Sure.

A. When I was looking at this table earlier, I wasn't sure about whether all of these other -- Chernobyl was certainly involved with producing electricity. I am not sure whether all of the other events that are associated technologies just producing electricity.

[4:25 p.m.]

Q. But they are energy-producing activities?

A. Your question was using the word electricity.

Q. Well, let's broaden it, then. Nuclear power is not an extremely dangerous form of

producing energy in comparison with other systems for producing energy on the basis of these figures?

A. On the basis of these figures.

Q. While we are on page 5-19, Mr. King, I think you went forward in the paragraph we were referring to to indicate that the authors of your exhibit have said:

> The future risk of severe accidents could only be predicted using the consequences of Chernobyl and its frequency of occurrence so long as Chernobyl is representative of the reactors and siting of concern.

And that is significant to you in the sense that you would not accept Chernobyl as being an appropriate reference for the risks associated with Ontario Hydro power reactors?

A. That's true.

Q. The authors go on to say: This has been shown in detail not to be the case for CANDU reactors, and refers to Snell and Howieson, and also the Ontario Nuclear Safety Review or the Hare Commission; correct?

A. Yes.

Q. Could we look at the Snell and

Howieson paper, which I think is at tab 11 of Volume 2. And I wonder if this might be given an exhibit number, Mr. Chairman?

THE REGISTRAR: Number 559, Mr. Chairman.

---EXHIBIT NO. 559: Document entitled Chernobyl: A Canadian Perspective.

MR. HAMER: Q. And it is the same Dr. Snell who wrote this paper, and, Mr. King, would you accept Exhibit 559, "Chernobyl: A Canadian Perspective", as being a useful and detailed discussion of the distinctions between the Chernobyl type of reactor and a CANDU type of reactor?

MR. KING: A. Yes, it includes that material.

Q. Sorry?

A. It includes that material, yes.

Q. If we go to page 2 at the bottom of the page we have a reference to the heart or core of an RBMK reactor - that is the Chernobyl type - consisting of a huge container about as big as a Canadian house filled with graphite blocks, and that is the point you were referring to earlier in connection with Dr. Hare's observation; correct?

A. Yes. Are you reading from a --

Q. Yes, I was reading from the bottom of

page 2 and going onto the top of page 4.

A. Oh, in the right-hand column.

Q. Yes, I'm sorry.

A. I was on the wrong column. Yes?

Q. And you see the figure on page 4, and is that a representative schematic of the Chernobyl type reactor, as you understand it?

A. Yes. I was looking at this figure over the last couple of days. It appears representative to me.

Q. And we see the graphite moderator in the left-hand side of that figure, the black squares?

A. Yes.

Q. And we see that the pressure tubes are connected directly to the heat transport system; is that correct?

A. I believe they would have a very similar arrangement to the CANDU reactors in that there is a feeder, a feeder pipe, a smaller diameter pipe connecting each pressure tube to larger piping in the heat transport system.

Q. But I thought you had told us in chief that the moderator and the heat transport system were separate in the CANDU reactor; is that right?

A. That's right.

Q. Whereas in the Chernobyl type reactor they are not separate systems?

A. No, the graphite is the moderator.

Q. I'm sorry?

A. The pressure tubes are inserted in holes which are bored in the solid graphite.

Q. Right. Right. But the water that is in the reactor core goes directly to the turbines in the form of steam; is that correct?

A. The water goes vertically up through the vertical pressure tubes and is turned to steam as it moves up the vertical pressure tube. And I mentioned that -- I believe there is something like -- well, there is in the neighbourhood of 1,600 pressure tubes, and each one of them would be connected with a feeder pipe - feeder means just a smaller diameter pipe to some larger piece of piping - in the heat transport.

Q. And to the turbines in the form of steam?

A. And that goes, as it shows in the figure, to a steam separator and then the steam from the top of the steam separator would go to the turbines, yes.

Q. Whereas in the CANDU system the steam that drives the turbine is separated from the heavy

water that is in the reactor core?

A. Yes. This is more like a boiling water reactor that was described earlier on. It doesn't have that intermediate loop that a pressurized water reactor or a pressurized heavy water reactor has.

Q. And on page 5 Dr. Snell writes in the left-hand column, second sentence:

> In the CANDU reactors where the moderator water is separate from the cooling water the moderator heat is removed by an independent moderator cooling circuit..., et cetera.

Then he goes on in the next sentence:

> The coolers keep the moderator temperature at about 70 degrees Centigrade or the same as from a hot tap. Obviously, you can't do that with solid graphite. In the RBMK design the graphite operates at a high temperature, about 700 degrees Centigrade, and if you could see it it would be glowing faintly red hot.

Is that your understanding as well?

A. Yes, it is.

Q. And he says towards the bottom of

that paragraph:

> The problem with graphite at high temperature is that if exposed to air it will burn slowly just like the charcoal briquettes on a barbecue, so it is very important in the RBMK design to keep air away from the graphite.

A. Yes. That's right.

Q. And they do that by filling the metal container with inert gases, helium and nitrogen. That is what he says in the next few lines?

A. Yes.

Q. And then if we look at the figure on the right-hand side on page 5 we see there is an upper shield in the middle of the Chernobyl reactor with the pressure tubes attached to that shield; correct?

A. I see that, yes.

Q. That is your understanding of the structure of the Chernobyl reactor?

A. Yes, I think this is accurate.

Q. And then over the page Dr. Snell discusses the idea of defence and depth that we have discussed earlier: prevention, mitigation and containment. I am paraphrasing what he says on page 6.

And then he says on the right-hand column

in the last paragraph on page 6:

> The Chernobyl Unit 4 reactor had shut down an emergency core cooling but had only a partial containment.

If we look at the figure at the bottom of that page we see that at the top of the reactor above that shield we were looking at there is a large industrial building as opposed to a containment structure; correct?

A. Yes, I see that.

Q. That is your understanding of that part of the Chernobyl structure?

A. Yes, I would add a comment that I am not familiar with all the details of the Chernobyl physical arrangement and structures. I get my information from figures like this, not from detailed design drawings.

Q. But it is well known, is it not, that one of the big problems in the Chernobyl accident was that the reactor had only partial containment?

A. Yes. In fact, the capability of the containment it did have I think was limited to the smaller range of loss of coolant accidents.

Q. The containment it did have was underneath and not on top of the reactor core; correct?

If I could direct you to Dr. Snell's

discussion on the right-hand column of page 6 about five lines from the bottom of the text:

> The pipes below the reactor core were inside what the Soviets called leak tight boxes. These boxes were connected to a huge pool of water under the whole building, the bubbler pond, as the Soviets named it. If one of the pipes in the boxes broke the steam would be forced into the pond where it and any radioactive particles it contained would be trapped in the water and the leak tight boxes would hold.

And that was a form of containment underneath the reactor; correct?

A. Yes. Why I am hesitating a wee bit is that the design of the containment at the top of the reactor, I'm not quite sure of what exactly that looks like. It is shown on this figure, if you just looked at the cross-hatched sections, is that the whole core is outside of containment.

Q. Well, a release downward, though, would be contained within the cross-hatched section; correct?

A. Well, given that this figure doesn't

show the detail of what it looks like at the top of the core, I can't say. But forgetting about the core itself, just the piping going into the bottom of it, then I think that description you gave would certainly be accurate and that is what would happen.

Q. At the bottom of the column on page 7 Dr. Snell writes:

> But the RBMK is a huge reactor. There is a tall fueling machine at the top that replaces the uranium as it is used up so the building above the reactor is large, about 71 metres high. The Soviets felt that to put all this in a containment is difficult and costly. To put the bottom pipes in containment is easier, and this was done. So Chernobyl Unit 4 represented a compromise, i.e. containment on the bottom but not on the top.

Isn't that so?

A. That is what it says. I am not familiar with --

There is something in this drawing. They have the industrial building and then they have what looks like a concrete walls above the steam separator.

I'm not sure what the capability of that structure is.

Q. Well, it says at the top of the column on page 7, three lines from the top: All the steam pipes above the core were inside ordinary industrial buildings.

And an Ontario Hydro containment structure is not an ordinary industrial building, is it?

A. I just point out that the figure doesn't seem to be consistent. It is showing a single line, which is typically a single metal sheeting type of a building, rather than -- well, the double line normally refers to a concrete, walled building. But you can still have a concrete walled building which still has a small design pressure.

Q. Well, are you saying that you understand this document is indicating that there was full containment or are you --

A. No. I'm quite aware that there was not.

I am just having a little bit of difficulty with this particular drawing, and it is pointing to the building above the core as industrial, and they are using a single line technique for representing that rather than a double line technique

for representing some of the other parts of the building above the core.

Q. All right. Well, I don't think it is fair to ask you to interpret the diagram that you weren't the author of.

A. Well, that is what you are asking me to do, I think.

Q. If we could turn the page there is a description of the accident sequence at Chernobyl. Have you had an opportunity to review that description there?

A. I scanned it in the last couple of days, yes.

Q. We can see in the left-hand column on page 8 about 10 lines down in section 2.1.1 a description of the test which the operators wanted to carry out at Chernobyl.

If I can summarize that, as I understand it, they wanted to test a situation in which the ordinarily electricity supply to the power station was lost, and the reactor was shutting down, and they wanted to see whether the spinning turbines slowing down could be used to generate emergency electricity supplies for the short period of time that it would take to have the diesel generators come up to supply

emergency electricity; is that correct?

A. That is my understanding of the purpose of the test, yes.

Q. They had a 30 second interval or so there, and they wanted to see if they could use the spinning turbine to cover that interval so that there would be control power and electricity for other purposes; correct?

A. I'm not familiar with the 30 second figure, but otherwise I believe that is correct.

Q. We see in the middle of that paragraph I referred to:

> These diesels usually start up in 30 seconds, and for most plants this is short enough interruption to keep important systems going. For the Chernobyl reactor the Soviets felt this was not short enough and they had to have almost an uninterrupted supply.

So in an ironic kind of way they wanted to be perfect and they set this chain of events in motion.

A. They had performed this same test on one of the other Chernobyl reactors sometime earlier and were successful in showing this, I believe.

Q. And we see as well on the right-hand column on that page that there was some time pressure on the operators to carry out this test.

In numbered item 1 we see the test was scheduled to be done just before a planned reactor shutdown for routine maintenance. If could not be done successfully this time, then they would have to wait another year to be able to carry out the test. So they felt under pressure to complete it.

A. That is my understanding of the situation.

Q. All right. Then if we go to the right-hand table on page 9 there is a sequence of the events, and if I can try to boil it down the reactor was placed into a position of very low power operation very early on the morning of April 26th; is that correct?

A. Some of these times, there was a recent international meeting just a few months back in Vienna which got some new input from the Soviet Union and wrote a report on the event, and I know that some times have changed but I don't think they substantively changed the sequence of the accident.

Q. All right. So that early in the morning the reactor was in a situation of low, very low

power operation; correct?

A. Yes.

Q. And that, we see on the right-hand side under the "Comments", meant that this design was unstable with the core filled with water so that it was in an unstable condition at the time the operator started to carry out the test?

A. Well, it was at low power. My understanding that they had all - I forget whether it is six or eight of their recirculation pumps in operation, and they were very close, just at the threshold of being subcooled, where they were liquid but with just a little more energy in they could convert that liquid to steam.

So if that is what you are referring to being 'unstable', then I would agree with that.

Q. Well, the comment goes on to say:

Small changes in flow or temperature--

This is in the right-hand column?

A. That is what I was referring to, yes.

Q. --can cause large power changes--

A. Yes.

Q. --and the capability of the emergency shutdown is badly weakened.

And that is your understanding of the instability at

issue here at that point?

A. Yes, my understanding is that in fact the shutdown system was not designed to handle the possible reactivity insertions that could incur when in the low power condition, as they were.

[4:45 p.m.]

Q. If I recall correctly, they weren't supposed to be operating the reactor at below about 20 per cent capacity; is that correct, because of this instability problem?

A. Again, I forget the exact number. It's normally referred to as in megawatts thermal, that's the units that the Soviets normally use.

There were rules that prevented that or should have prevented that from occurring because there were special concerns from the designers about operating in that condition, yes.

Q. In fact, I think we see later on that they were operating in what is called a prohibited range of power.

A. Well, I don't recall exactly whether it was unadvisable or prohibited by procedure, or exactly the administrative control that they had on it. If it wasn't prohibited it was certainly highly undesirable.

Q. In any event, the next thing that happened is that the operator blocked the automatic reactor shutdown system and there was one trip where there was a low --

A. Where are you reading now?

Q. I am at 120.

A. I know that they were blocking certain trip parameters of the shutdown system. I don't believe they blocked the complete shutdown system.

Q. And they then turned off the remaining turbine which was running at 123 or thereabouts?

A. Yes.

Q. And the power started rising and the operator tried to shut the reactor down manually?

A. That's correct.

Q. But because the reactor was in this inherently unstable condition, exactly the opposite thing happened to what was supposed to -- or what he hoped would happen?

A. The power began to rise because it was in this condition, because when they shut off the turbine, you have lost your power supply to your recirculation pumps, you don't have as much flow, you

still have an energy input because you are at one per cent power, you created a void situation and that, with a positive void coefficient, led to an increase in power.

When the operator pushed the manual shutdown button, as it, I believe, is noted here on the right-hand side, it's generally accepted now I think that -- and as it's stated here, this was first established as a theory by reactor physicists at AECL, that there is what is called -- the shutdown system rods are designed such that the bottom part of them are made of graphite, and just because the condition that they were in, when the shutoff rods entered the core they in fact inserted positive reactivity instead of what they should have done is inserted negative reactivity, and in fact that was a positive trip, sort of thing, and that once that occurred then the reactor power increased at a very high rate.

Q. And those shutdown rods are rather different in operation, as I understand it, from the CANDU shutdown rods; is that correct?

A. They are very much different. They are very much slower and they have this particular feature, this graphite trailer on the end.

Q. And they move at something like a

half a metre a second or something like that?

A. I don't know the exact rate, but I know they are very much slower than the Canadian rods.

Q. And that was the end of that, about 4 seconds later, and the reactor power reached about 100 times the normal full power and there was excess steam pressure which broke the pressure tubes, and that caused the top shield with all the pressure tubes attached to it, that we looked at earlier, to blow off, breaking all the rest of the pressure tubes?

A. The initial sequence was that some pressure tubes failed, but since I believe that part of the containment was only designed to handle the rupture of two pressure tubes, it caused the top of that reactor, the top of the reactor design, where all those individual pressure tubes to go through, to raise up and in doing so it ripped off all the connections with a large number of the other pressure tubes. And therefore, what started out as a small loss of coolant accident became a very large loss of coolant accident, and that in turn would have augmented the power runaway because now you are having a much faster blow down in the core.

Q. And if we could look to the page 11 then, section 2.2.1, we see the power surge destroyed

the top half of the reactor core, the building immediately above the reactor and some of the walls on either side. That is the ordinary industrial building that we were looking at in one of the earlier figures; correct?

A. I believe so.

Q. And then with that building destroyed, burning fragments of fuel and graphite were thrown out and landed on the roof of the adjacent building, causing about 30 fires.

A. That's my understanding.

Q. And if we look to the top of the right-hand column on page 11, Dr. Snell writes:

> The destruction was not of course caused by a nuclear explosion, but by steam and perhaps chemical explosions.

So the damage was confined to Unit 4, is that your understanding as well?

A. Well, as I mentioned earlier, I am not aware of the significance of the steam explosions, but I am not saying that this is wrong. I just may be unfamiliar with that part of the sequence of the accident.

Q. And then if we can go over to page 16, Dr. Snell has table 2 comparing the CANDU and

Chernobyl designs, and he is referring here to CANDU 6, but I don't think that in principle, at least, you will find any significant difference with the Ontario Hydro stations.

Do you have that table there? I'm sorry, page 16.

A. 16, yes. Okay.

Q. And we see some of the differences being that the coolant in Chernobyl is ordinary water, CANDU is heavy water. We have referred to the difference in the steam cycle, Chernobyl is a direct steam cycle from the reactor core, CANDU is indirect.

A. Yes.

Q. And the moderator is graphite bricks in the one, heavy water in the other.

And then under safety systems we have talked about containment being incomplete in Chernobyl, and with CANDU you either have a common containment building or a containment structure which surrounds the individual reactor; correct?

A. Yes. All the heat transport piping is within a defined containment boundary.

Q. In Chernobyl the shutdown mechanism is a single mechanism with the absorber rods which you were referring to a moment ago?

A. Yes.

Q. And any future CANDU station would have two complete system consisting of absorber rods and liquid injections?

A. Yes.

Q. And it's effective in two seconds as opposed to ten seconds for the Chernobyl shutdown system?

A. Again, I am not familiar with the exact number on the Chernobyl, and the CANDU my understanding is it's less than two seconds.

Q. And in the Chernobyl shutdown system the system is not independent of the operation of the plant to generate electricity, whereas in CANDU the shutdown system's effectiveness is independent of the normal operational systems; is that correct?

A. As in Chernobyl and with some other types of reactors, there is usage of control rods. When I use the term control rods, that's the use for control of power in the normal power production mode, and those rods are used both for control in normal production and for the emergency shutdown situation, where that's forbidden by AECB rules in the Canadian licensing approach. There is a complete separation of the devices used to control power in normal operation

and those devices that are used to control power for the emergency shutdown system.

Q. And the difference in the effectiveness of the shutdown systems is a critical difference between CANDU and the Chernobyl type reactor; is that fair?

A. Both the effectiveness and the independence between normal power producing systems. Process system is the terminology we use.

Q. And then if we could go over to page 19 there is a discussion in the right-hand column of something you mentioned a fuel called positive void coefficient, and that is an aspect which is common to the CANDU and Chernobyl reactors; correct?

A. Yes.

Q. And if I can try and put it into my simple-minded language. The positive void coefficient is the term for the characteristic of the reactor in which the reactor becomes more active when there are steam bubbles formed in the moderator, or, indeed, if the liquid turns to steam.

Is that a fair --

A. Not really.

Q. Well then, I am going to let you go at it.

A. There are three important reactivity coefficients in a CANDU reactor or in another type of reactor, they would be a density coefficient in the moderator, a density coefficient in the coolant, and a fuel temperature coefficient.

In a light water reactor, of course, which has the moderator and the heat transport as one, then there is just the one coefficient.

In a CANDU reactor, what we are talking about is the positive void coefficient. Whether it is called a density coefficient or void, it's really the same thing. A little more general use of the term to call it a density coefficient.

What happens is if the density decreases, that increases the reactivity for the fission process, and that will increase power.

Now, whether that density decreases due to void creation or whether it is just due to a rise in temperature of the heat transport coolant, then that is a positive void coefficient.

If it was the other way around, if an increase in density occurred, and if that lead to -- that would, in a CANDU reactor, would lead to a negative insertion of reactivity.

So, it is really when we talk to the

subject of void coefficient, it's really referring to - not the moderator as you had indicated - but really only to the heat transport coolant.

Q. And if we look at the right-hand side of page 19, Dr. Snell writes:

> Other reactors such as U.S. water cooled reactors have the opposite effect, the power goes down as the boiling increases...

And we should substitute the creation of void for boiling in that sense, I take it.

So that in a light water reactor, if you have a decrease in density, the power goes down which is a negative void coefficient; is that correct?

A. But then if you have an increase in density in that type of reactor, you would have a positive reactivity effect.

Q. And that was the point I wanted to come to, which is that you could have a system failure which could cause an increase in density and that would cause increased reactivity?

A. In a pressurized water reactor, if you have an over-cooling transient, if the feed water that goes into the steam generator all of a sudden it starts coming in cooler than it should, it cools down

the heat transport system, increases its density and that's a positive reactivity insertion into a pressurized light water reactor.

In a boiling water reactor, as the type that General Electric, for example, markets, where there is boiling in the core, they have an accident called an isolation accident, and since the water from the core leaves containment and goes to a turbine, similar to the direct cycle of the R&BK, there is an isolation valve as that leaves containment, and if that value of closes spuriously, it causes an increase in density. It's collapsing of the voids in the boiling water reactor and that is a positive reactivity insertion as well.

Q. And the point in either case whether you have a positive or negative void coefficient is to have an effective and rapid-acting shutdown system to deal with either type of accident; is that fair, or that's one of the points?

A. Your safety systems have to be designed to reflect the characteristics of the accidents you are trying to protect against.

So in our case where we have a positive void coefficient, which we have to be extra careful on the shutdown systems, and that's why the rules have

developed for having the shutdown systems that we have.

Q. And then the last point on this document, and I will stop, Mr. Chairman, is that because of the independence of the safety systems, safety shutdown systems in CANDU, Dr. Snell observes on page 20 under the heading The End Result, he says at the end of that paragraph:

> In a CANDU, the capability of the safety systems is independent of the operating state. As well, we have much more backup systems especially for shutdown. In that sense CANDU is a much more forgiving design.

And you would agree with that characterization, I take it?

A. Well, I am hesitating on what he means by the word "forgiving". If he is meaning it can allow for a design -- or system failures or operator failures, and I guess he is just referring in this sentence to the -- no, sorry. He is referring to all safety systems.

Well, I would say given that the whole licensing approach where we have our dual failures, where we have to analyze a process system failure and the failure of one of the special safety systems, just

by doing that, then we are providing for a more forgiving design and that it can handle the failures of safety systems. If that's what Dr. Snell is referring to, I would certainly degree with that.

Q. I should have started earlier in the paragraph, he is describing the Chernobyl characteristics, and he says about four lines down:

> As we have seen, its safety depended very heavily on operators staying within certain limits.
>
> And we talked about that earlier.

[5:06 p.m.]

> If the operators went outside those limits the safety systems could be ineffective in an accident and in a very real sense the operators would be operating blind.

A. Well, over the time since '86 when Chernobyl occurred the allocation of blame towards designers and operators, that balance has changed somewhat--

Q. Shifted a bit?

A. --over those years. This is an '88 report, and I think the shift has been to make a more equitable allocation of blame towards the designers and

the operators, not just strictly on the operators as perhaps was reflected by international opinion at this time.

So whether they should have been in that stage or whether the design should have provided shutdown systems which could have handled the transience that occurred when you were at low power, and therefore, it was more of a design error rather than an operating error, but it is a combination of errors in both areas.

Q. In other words, the design on that side should have been more forgiving than it was?

A. Yes. The opinion right now is, yes, the design of the shutdown systems should have been a lot better in Chernobyl.

MR. PENN: A. One other important point, we don't rely on steam turbine rundown for power supply in those circumstances.

Q. We should add that to that table then, shouldn't we.

A. If you will.

THE CHAIRMAN: How much longer do you think you are going to be?

MR. HAMER: I hope to finish by late morning tomorrow, Mr. Chairman.

THE CHAIRMAN: You are next up, Mr. Bullock, are you?

MR. BULLOCK: I am, sir. If Mr. Hamer is done by mid-morning tomorrow hopefully I can be done by the close of business tomorrow; if not, perhaps into early Monday morning.

THE CHAIRMAN: So are you saying unless Mr. Hamer finishes by mid-morning tomorrow you will be at least the rest of the day?

MR. BULLOCK: I am saying about a half a day to three quarters of a day, I expect, sir.

THE CHAIRMAN: Okay. Thank you. We will adjourn until tomorrow morning at ten o'clock.

THE REGISTRAR: This hearing will adjourn until ten o'clock tomorrow morning.

---Whereupon the hearing was adjourned at 5:10 p.m. to be reconvened at ten o'clock on Thursday, April 2nd, 1992.

E R R A T A
and
C H A N G E S

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Date: Thursday, January 23rd, 1992.

Page No.	Line No.	Discrepancy
v & 18670		2.29.47. s/r 7.29.47.